

An Expensive Way to Shut a Stable Door

It is permissible that governments should change their minds from time to time, and it is even credible—although only just—that Mr John Davies, Secretary of State for Trade and Industry, should have managed to keep his job in the past two years while having pinned his reputation, first of all, to the doctrine that the government to which he belonged would not help lame ducks and then having graduated as one of the biggest glad handers of this unhappy generation. Everybody knows that the present British government would have been happy enough to allow all kinds of ailing enterprises, the Upper Clyde Shipbuilders, for example, to follow Rolls-Royce Limited into bankruptcy if it had not discovered, rather late in the day, that such a policy is politically tenable only when unemployment is not rife. In that sense, it may be held that Mr Davies and his policy on lame ducks have been overtaken by political events for which he himself is not directly to blame. But none of this excuses the way in which Mr Davies announced this week that the government is now proposing to lend International Computers Limited £14.2 million against the vague and unmarketable promise of royalties on future sales and with the objective of allowing the company to continue with its research and development programme. At the same time, the British government has let it be known that financial control of ICL will have to remain in British hands and has thus echoed General de Gaulle's declaration a decade ago that Machines Bull must remain forever French.

International Computers Limited is already a much favoured commercial enterprise. Its recent history has been powerfully shaped by the now defunct Industrial Reorganization Corporation, which managed to effect a marriage between what used to be International Computers and Tabulators Limited, itself a descendant of Ferranti interests in civil computers, and the smaller and still less successful enterprises owned by The English Electric Company Limited, now also merged in a larger company. Over the years, and supposedly in the interests of a strong economy, ICL has enjoyed benefits ranging from open subvention to most favoured status in the sale of computers bought with public funds. By now, shareholders must be puzzled to know precisely what they own and what its commercial prospects may be. Occasionally they are told of exciting new projects in development which may help to leapfrog over the competition of other giants in the field, IBM in particular. On other occasions, they are given a diet of cold water as when, earlier this week, they were told that profits had fallen in the first half of 1972, and that no interim dividend would be paid. There seems very little doubt that the government's decision to help the company out of its immediate difficulties has been provoked by a bid for a controlling interest by the American Burroughs group.

Ten years ago, the present policy might have been justified. Then there was good reason to believe that there would be great commercial advantages accruing to

those who could manufacture electronic computing machinery efficiently and cheaply. Time has proved that this prediction was correct. In the United States, several companies, not merely the giants, have done wonders for themselves. But the days have gone when the manufacture of hardware was the most obvious source of benefit in the application of computers. Now, by painful experience, it has been acknowledged that software is at least as important. Moreover, for countries such as Britain, the most obvious economic benefits are to be won in the efficient application of computer systems to commercial needs of various kinds. By comparison with the advantages to be derived in ways like these, the profit to be won from exporting computers in tough competition with companies such as IBM must surely be small. Moreover, there is at least a chance that a sensible merger with overseas interests might be arranged in such a way that Britain would become an even more successful exporter of computer hardware and that the unemployment figures on which the government quite properly broods each month would if anything be reduced. What this implies is that the further subvention of £14.2 million now offered to ICL may be less of an investment in the country's economic success than an impediment to the kinds of development which are proper and likely to be profitable in the 1970s.

The dilemma over the financing of ICL is yet another indication that the British government is desperately in need of a mechanism for financing industrial innovation. In the past few years, all kinds of devices have been tried. The old idea that the National Research Development Corporation might have a useful part to play, valid though it may have been in the 1950s, has been undermined by the recognition that market is at least as important as invention. Government departments and research organizations have from time to time been urged to take a leaf out of the American book, and to place contracts in industry that would bring commercial advantages in the civil field, but these have usually been defeated by the unwillingness of public servants to see that commercial interests should profit from public expenditure of any kind. The notion that government laboratories such as the Atomic Energy Authority laboratory at Harwell might function as a source of scientific research and development for industry is laudable but of limited application. At the same time it is clear that government support for cooperative research associations is at best an archaic device. Yet technology abounds with ways in which the efficiency of industry and the saleability of its products might be increased by long and medium term investment in research and development. The trouble is that such activities need planning and that the work must be done not by Treasury officials but by technical people. One of the few attractions of Mr Anthony Wedgwood Benn's now defunct green paper on government research and development is that it did at least suggest a mechanism



by means of which industrial research of a long-term character might be intelligently sponsored. It will be interesting to see how long it will be before Mr Davies's changeable ministry stumbles on a similar solution.

Indian Science

UNESCO, a notorious source of uncritically analysed information, has produced a non-report on the organization of scientific research in India (*National Science Policy and Organization of Scientific Research in India*, obtainable from HMSO, £0.75). So far as it goes, the document is a useful compilation of the way in which the government of India organizes its support for laboratories, both in the public domain such as the laboratories of the research councils and those of the Atomic Energy Commission and also in university research laboratories. Unfortunately, however, the document skates neatly around the issues which at present preoccupy not merely the government but those in India who work in scientific research or who aspire to do so, and who at present labour under a heavy burden of frustration. The suicide of Dr V. Shah some weeks ago (see *Nature*, **237**, 130; 1972) was a poignantly unnecessary and melodramatic proof of what many young men and women in India consider to be the intolerable restraints under which scientific work must be carried out, and the issues have been underlined by recent correspondence in *Nature*. In the universities, there are too few opportunities for research, too little money and too many university departments in which an established and sometimes ageing head casts a shadow on the work of his colleagues or subordinates. Then there are many government laboratories in which the bureaucracy serves to stifle what energetic people might accomplish.

The other side of the coin is much more cheerful. There is very little doubt that the frustrations of Indian scientists are a function not so much of the way in which scientific research as such is organized as of the traditions which have, over centuries, given Indian public life its characteristic pattern. It is also proper to acknowledge that the seeds of change are now being widely sown. For more than a decade, the Atomic Energy Commission has provided a splendid demonstration of the productivity that can spring from a civilized egalitarian spirit in laboratories, and that is only one of the Indian research institutions which have been able to break new ground. Moreover, the government of India seems by now well seized of the need to appoint as heads of its research councils people who will foster change. In the past year, both the Council for Scientific and Industrial Research and the Agricultural Research Council of India have acquired executive heads whose reputations will depend on the speed with which they can transform the spirit of their institutions, making them more vigorous and adventurous.

There remains the question of what scientific research is for in India. In past decades, there has been a tendency to assume that pure scientific research, the stuff of which Nobel prizes are made, is the best of all research and, as a result, too many laboratories have tended to have their feet well above the ground. Undoubtedly, there is a case for more closely directed applied research, but there is a danger that recent

attempts to make laboratory research programmes show the promise of practical and measurable benefit may have gone too far. Certainly it would be anomalous if at this stage in the development of the economy of India, the funds available for research were to cease to grow more quickly than the Gross National Product, and it is doubtful whether the attempts of recent years to use scientific research as a means of replacing imports from abroad is a sensible use of manpower.

What Next with Whales?

THE annual conference of the International Whaling Commission in London last week will no doubt have been a severe disappointment for those who looked to the meeting for a ten-year moratorium on whaling of all kinds. Nobody would wish to defend the IWC against the charge of complacency, for the timorous record of the past few years is nothing short of an international scandal. But on political as well as scientific grounds, a complete moratorium on whaling would have been neither practical nor desirable. The most telling proof of this is that the International Whaling Commission does not at present include a number of important or potentially important whaling nations, Chile and Peru, for example, not to mention China. Even if the fourteen members of the IWC had been able to persuade each other that there should be a moratorium, they would have been unable to influence the policy of the absentees. In short, the demand that the IWC should enforce upon its member nations a complete moratorium is as unrealistic as the demands which are made in other fields that national governments should go in for unilateral disarmament. Pressures like these may ease the consciences of those who exert them, but they are unlikely to accomplish more than that. But since the long-term conservation of whales is an important and worthy cause, the best strategy will be to induce whaling nations not at present members to join the IWC. In the meantime, it is something to be grateful for that the conference last week agreed to tighten up on quotas and to define quotas separately for different species, that it agreed to give the international observer scheme more teeth and that there is now a prospect that the IWC itself will be made a more effective organization (see page 4).

In retrospect, it is hard to understand why enthusiasm for a total moratorium should have been so eagerly endorsed at the Stockholm Conference on the Human Environment and then carried to London by the delegations of responsible governments, especially that of the United States. The scientific committee of the IWC which met in London on June 19, says categorically in its report to the conference that "a blanket moratorium cannot be justified scientifically". The committee points out that in many ways a moratorium would suffer from the defects of the old and much maligned regulations which allowed a whaling ship to calculate its quota of catch in terms of a blue whale unit. Both devices are attempts to regulate several stocks as a single group, but prudent management should be based on separate regulations of individual species. But there is also a more practical objection to a moratorium. Everybody agrees that there should be a better understanding of the whale stocks, but as things are, most information

comes from the study of the whales which are caught by whaling vessels. How else, for example, is it possible to estimate the age distribution of some specific stock of whales? For the time being, at least, carefully controlled hunting of less threatened species is ironically a necessary preliminary to the development of an effective conservation policy.

What else needs urgently to be done? The London conference appears to have echoed the popular demand for more research, and the scientific committee has made some progress in mapping out a programme. The need now is to ensure that the IWC will make this effective as quickly as it can. To begin with, it must set out urgently to persuade non-member nations to provide more complete information about their whaling. Statistics of catches, together with estimates of age distribution, are an urgent need, and the time has come to extend this to the smaller species of cetaceans, dolphins included.

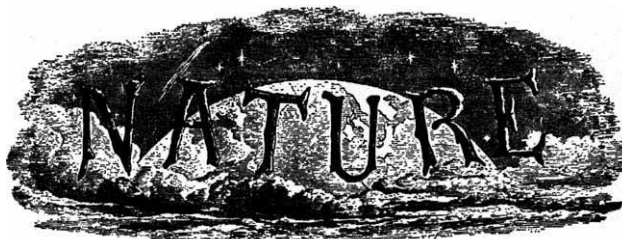
A more vigorous programme of marking whales is also necessary. The best estimates of whale populations are at present based on sightings by whaling crews, but in the long run a much better understanding of the age distribution of the whale population and of its migratory habits will come from the recovery of marked animals. Yet in the past two years, fewer than 500 whales have been marked. Since marking began in the 1920s, it is unlikely that more than 15,000 animals of all species have been treated in this way. The result is that the rate of recovery of marked whales is too small to allow much useful information to be gathered. There is a strong case for putting at the disposal of the IWC vessels whose sole function would be to mark whales of particular species, not necessarily during conventional hunting seasons. There should be a target of several thousand markings each year, while the development of sonic and radio marks should be pushed ahead.

Much of the recent justifiable complaint against the IWC has been levelled at its lazy habit of attempting to control the whale population as a whole. It is now, however, necessary not merely that different species should be separately conserved but that there should be a better understanding of how individual species are composed of distinct breeding stocks. On the basis of the past hunting of blue and fin whales, for example, it is known that there are separate populations which, in the southern hemisphere, are confined to the Indian Ocean, the Pacific and the Atlantic. It is important to discover whether similar rules apply to other species, sei whales for example. The most promising line of investigation is the identification of separate breeding stocks by means of biochemical techniques based, for example, on the proportions of different enzymes in the blood and other tissues.

There remain ecological problems which are daunting in their difficulty but which, nevertheless, must be solved before a sensible conservation policy can be formulated. Although blue whales appear to be almost entirely dependent on crustacean krill for food, the diet of other species is less well determined. Little is known of the factors which regulate the production of nutrients in the oceans and, in the circumstances, it is no surprise that nobody knows whether the decline of some whale populations in the past few decades, and the greater availability of food which this implies, has led to an increase in other populations, dolphins or porpoises, for example,

or even other pelagic species. It is true, of course, that any systematic attempt to investigate these problems would be expensive. In the long run, however, it would be an immense benefit not merely to whales, but also to the whaling nations.

100 Years Ago



TO THE RIGHT HON. W. E. GLADSTONE,
First Lord of the Treasury, &c. &c.

We, the undersigned, deeply interested in the condition of English science, and viewing with special concern the treatment which the eminent Director of the botanical establishment at Kew has systematically received at the hands of Mr. Ayrton since his appointment to the office of First Commissioner of Works, do most respectfully beg your attention to the following statements and observations.

In the year 1840 the private Botanic Gardens of Kew, which had previously been in the possession of the Royal Family, were handed over by the Queen to the Government.

A commission then appointed to report on their condition recommended that they should be enlarged and maintained as a national scientific establishment, which should form a centre of reception for the useful products of the vegetable kingdom, a centre of reference and distribution for England, India, and the colonies, and a means of augmenting the rational pleasure, increasing the knowledge, and refining the taste of the English public.

It but rarely falls in either with our duties or our desires to meddle in public questions; and not until we found Dr. Hooker maimed as regards his scientific usefulness—not until we saw the noble establishment of which he has hitherto been the living head in peril of losing services which it would be absolutely impossible to replace; not, indeed, until we had observed a hesitation upon your part which we believe could only arise from lack of information—did the thought of interference in this controversy occur to us. Knowing how difficult it must be for one engrossed in the duties of your high position to learn the real merits of a conflict like that originated by the First Commissioner of Works, we venture to hope that you will not look with disfavour on an attempt to place a clear and succinct statement of the case before you.

That statement invites you respectfully to decide whether Kew Gardens are or are not to lose the supervision of a man of whose scientific labours any nation might be proud; in whom natural capacity for the post he occupies has been developed by a culture unexampled in variety and extent; a man honoured for his integrity, beloved for his courtesy and kindness of heart; and who has spent in the public service not only a stainless but an illustrious life. The resignation of Dr. Hooker under the circumstances here set forth would, we declare, be a calamity to English science and a scandal to the English Government. With the power to avert this in your hands, we appeal to your justice to do so. The difficulty of removing the directorship of Kew from the Department of Works cannot surely be insuperable; or if it be, it must be possible to give such a position to the Director, and such definition to his duties, as shall in future shield him from the exercise of authority which has been so wantonly abused.

CHARLES LYELL
CHARLES DARWIN
GEORGE BENTHAM, Pres. Linn. Society
HENRY HOLLAND, Pres. Royal Institution
GEORGE BURROWS, Pres. Roy. Coll. of Physicians
GEORGE BUSK, Pres. Roy. Coll. of Surgeons
H. C. RAWLINSON, Pres. Roy. Geogr. Society
JAMES PAGET
WILLIAM SPOTTISWOODE
T. H. HUXLEY
JOHN TYNDALL

From *Nature* 6, 211, 215, 216 July 11, 1872.

OLD WORLD

Pyrrhic Victories for the Friends of Whales

THE annual meeting of the International Whaling Commission ended last week with Professor J. L. McHugh (its chairman) in high spirits, its opponents (the environmental lobby) in high dudgeon and its business (the whaling industry) still very much a going concern.

At the end of the commission's four day session, at which the plan for a total moratorium sank without trace, McHugh described the meeting as "perhaps the most successful the commission has ever had". Declaring that "this has finally got the whaling commission where it ought to be", he listed as the meeting's achievements

- a complete and indefinite ban on the taking of blue, right, gray, bowhead and humpback whales;
- the introduction of international observers on all whaling ships;
- the abolition of the blue whale unit;
- the reduction of quotas on endangered species, and the introduction of one for the minkie whale;
- the creation of a committee to examine IWC's procedures and to strengthen them.

How well is Professor McHugh's enthusiasm justified? The hoped-for 10 year moratorium—supported as it was by a motion at Stockholm that was carried by an overwhelming majority—went down six votes to four, with four abstentions. The abstentions included Canada, France and Denmark, who voted for the moratorium in Stockholm.

The total ban on bowhead, right and gray whales was introduced more than twenty-five years ago, and blues and

humpbacks have been protected since 1967 and 1966 respectively. What this meeting of the IWC has done is to extend the protection indefinitely, instead of for renewable periods of between three and five years.

The International Observer Scheme was first mooted in 1955, but even now it is not truly international; the larger whaling nations simply exchanging observers.

The blue whale unit, now finally abolished, has been the IWC's biggest curse. It equates 1 blue whale to 2 fin whales to 2.5 humpback whales to 6 sei whales, and catches in the Antarctic have been estimated in these units. As a result, the larger whales are hunted to near extinction before the smaller whales are sought, as about the same amount of effort goes into catching a sei whale as a blue. The abolition of this arithmetic was first suggested in 1954, and has been abandoned only now that the blue and humpback whales have to be completely protected and when fins are heavily depleted.

The reduction of quotas, giving the numbers in Table 1, is a move in the right direction, but the new quotas are a mere 2,500 or so below the actual catch for last year. And the simple figures hide certain facts. The fin whale is heavily depleted, and for stocks to reach maximum sustainable yield (MSY)—the level at which the maximum number can be taken every year *in perpetuum*—it has been estimated that a complete moratorium on the species would be needed for 25 years. At the level of hunting now proposed, about 60 years would be needed for stocks to reach the MSY. Again, the quotas restrict catches only

in certain areas; as many minkie whales as any country wishes can be taken from the North Pacific, for example, and any species not protected or named in the table can be taken at will.

What crumbs of comfort can be picked from the IWC's table? Precious few, but there is some hope for the future. The Japanese and Russians have agreed that quotas should be further reduced next year, and the United States and the USSR agreed to cooperate over whale research. A proposal for an International Whale Decade to enlarge the sketchy knowledge of whale habits was approved in principle. Attempts are to be made to get the non-IWC whaling nations — Peru, Portugal, Spain, Brazil and Chile—who between them account for between 2,000 and 4,000 whales a year, to join the IWC, and the procedure committee is to look at ways of giving the organization some teeth. In the past its bark may not have been particularly strong, but its bite was non-existent.

There is also some comfort in that the quotas now set for sperm, sei and minkie whales are at the theoretical MSY level. But there remains great uncertainty about the accuracy of the calculations. Estimates of whale populations are based on sightings from killer ships which naturally go to the areas where experience has taught them whales are to be found, so their accuracy as total population estimates is questionable. A decade of research on the subject should provide figures from which MSY levels could be set with certainty. Meanwhile the danger is that, while a fishery if overexploited can recover within a few years, whales, with their slow mammalian reproduction cycle, can take decades to recover.

Mr Russell Train, chairman of the United States Council on Environmental Quality and head of the American delegation, said last week that "there's room for improvement but it's not been a bad week". But "we are managing by guess and by God, rather than on the basis of fact".

ROTHSCHILD

Shot Across the Bows

THE customer-contractor principle is not necessarily the universal answer to the organization and management of government research and development,

Whale Quotas for 1972-73

Area	Species	Quota	1971 catch ¹	1971 quota
Antarctic	Fin	1,950	2,683	2,300 BWU*
	Sei	5,000	5,456	
	Minkie	5,000	3,020†	
North Pacific	Fin	650	732	1,046
	Sei	3,000	3,463	3,768
	Sperm (male)	6,000		
	(female)	4,000	7,328‡	10,841‡
Southern Hemisphere	Sperm (male)	8,000		
	(female)	5,000	10,796‡§	No quota
Total		38,600	41,000 (approx.)¶	

¹ Source: Bureau of International Whaling Statistics, Norway.

* Blue Whale Units.

† Not included in the Blue Whale Unit.

‡ No separate quotas by sex.

§ Includes Sperm whales caught by non-IWC countries.

¶ Includes all large whales in all areas. Does not include several thousand small whales and more than 250,000 dolphins which are not protected in any way.

according to the Select Committee on Science and Technology. This statement, which is in direct conflict with Lord Rothschild's philosophy on the subject, is the nub of the committee's third report on aspects of government research and development published yesterday (*The Industrial Research Establishments of the Department of Trade and Industry*, HMSO, £0.235). The select committee feels that the principle is especially inapplicable to the standards work undertaken at the National Standards Laboratory. The committee does, however, reluctantly admit that "particular areas of the work of the industrial research establishments may be susceptible to this principle".

The select committee also has reservations about the requirement boards now being set up by the Department of Trade and Industry. These boards, which are designed to increase the relevance of government research and de-sequence of the acceptance by the DTI a few months ago as a direct consequence of the acceptance by the government of the customer-contractor principle. The select committee says that insufficient thought has been given to these boards and that they will only function satisfactorily "within the framework of a coherent policy"—which, it says, does not exist.

The committee also expresses concern about the terms of reference of the requirement boards. Mr Michael Heseltine, Minister for Aerospace, said in evidence that the tasks of the boards will be decided when they are set up and the committee naturally thinks that this is putting the cart before the horse. The committee also considers that the government is unprepared for the boards—it has not, for example, been decided whether board members should be paid.

The conflict between the select committee and the Confederation of British Industries (see *Nature*, 237, 477; 1972) is again apparent. The select committee recommends that the industrial research establishments should be more competitive and should continue to provide supporting scientific services and technical facilities which industry "cannot or is unlikely to establish on its own account". The CBI, on the other hand, reaffirmed before the committee its belief that research and development for industry should be done within industry itself and that government laboratories should "do most of their work for the government and its agencies".

The select committee, always a sensitive index of public opinion, panders to the environmental lobby by suggesting that the industrial research establishments should "devote more attention to exploring the impact of technology on the community and to solving environmental problems". The committee also suggests that the laboratories should

have more independence although it was admitted in evidence that even at present a director of one of these establishments is "very much the captain of his own ship".

This independence could be achieved, according to the select committee, by setting up a Research Establishment Authority which would include the National Physical Laboratory, the National Engineering Laboratory and the Warren Spring Laboratory. These are the DTI establishments which, according to the department, carry out programmes "for the benefit of industry in pursuit of the department's objective of assisting British industry and commerce to improve their economic and technological strength and competitiveness". The purpose of the authority would be to supervise the work of these laboratories and to "enable them to effectively transfer their technology to industry".

SELECT COMMITTEE

Population Policy Ahead?

THE British Government continued to hedge last week on a population policy. Giving evidence before the Select Committee on Science and Technology, Mr Robert Carr, the Lord President of the Council and the minister responsible, said that policy would have to wait until the Population Panel had reported at the end of the year. The panel is a body of five civil servants (one a woman) and two academics, Professor Michael Abercrombie, Director of the Strangeways Research Laboratory, Cambridge, and Mr G. B. Richardson, of St John's College, Oxford. It was set up last November, after a select committee report in May 1971 calling for a special office for population and a declarative population policy. The government appears to agree that the matter is "urgent" but that not enough is known about it. During the hearing last week, it emerged that the panel is not examining how a population policy should be implemented or even what that policy should be, but simply whether one is necessary.

The select committee was unimpressed. Why did it take five months from the government's announcement of the panel in July 1971 to the appointment of its members? Why had it taken the panel a further month to meet? And when might action be expected? Mr Carr offered the innocent defence that action had not been slow by government standards. The panel is to report about the end of this year, and Mr Carr promised action before the end of 1973.

The select committee also complained about the composition of the panel.

Why are only zoologists and statisticians represented on a panel dealing with human population—why not a doctor? And why not a second woman? Why not a female doctor, even? But Mr Carr was not to be persuaded. Adding someone at this stage, he said, would slow down the work until the newcomer caught up. In any case, he said, "people can look wider than either their sex or their expertise".

Mr Carr and the select committee also crossed swords over publication of the panel's putative report. Publication could not be guaranteed, said Mr Carr, although he would need "a pretty good reason" not to publish.

The committee had no better luck from Mr C. R. Ross, the panel's chairman. Will the report be a policy document, the select committee asked? Mr Ross said that it would present the arguments on all sides, and he hoped that a policy would crystallize from that, but he would not call that a policy document. Attempts by the select committee to extract from Mr Ross the line of the panel's thinking were to no avail. The committee is still only half-way through, Mr Ross said, and to give an indication of what it plans to recommend would be premature. All he would say, in answer to persistent questioning, was that he thought that the panel would recommend a permanent body to study population growth. What relationship this will have with the offices of Actuary-General and the Registrar-General was not made clear.

Memory Lingers on . .

AT a Royal Society *conversazione* last week one of the exhibits included memory tests to question the myth that old people remember distant events better than recent ones. One of the questions was "Who was Guido Pontecorvo?". The alternatives on offer included racing driver, orchestral conductor and traitor. Pontecorvo is none of these. Guido Pontecorvo is a distinguished and respected geneticist at the Imperial Cancer Research Fund in London. His brother Bruno, to whom the question with its past tense presumably refers, is a nuclear physicist. He left Harwell in 1950 for a higher post in a nuclear research laboratory in the USSR, at a time when the sale of British nuclear secrets was hot news. To be sure, Pontecorvo left without notice while on holiday in Italy, but the charge of traitor was never formally levelled, and no attempt was made to bring a case. Perhaps a few old memories need adjusting.

COMPUTING

Expansion Planned?

A THREE-FOLD increase in the support of computing by the SRC between now and 1975-76 is recommended by the Computing Science Committee of the council. This encouragement to computing in British universities is intended to produce increased support for research, an expansion in postgraduate courses, a broadening of the activities of the Computer Board of the SRC and a substantial strengthening of the academic staff in computing departments. (*Computing Science Review*, SRC.)

The changes recommended by the committee imply a level of support for computing in universities of £2 million in 1975-76, but the committee warns that this might not be possible for lack of manpower. In the long run, however, the committee considers that manpower should be no problem. Of the 210 research students expected to qualify in 1978, only 52 will be required to maintain the expansion of the university departments. A higher percentage, 18 out of 50 qualifying PhDs and MScs, will be needed in 1973.

No hard and fast recommendation is made as to how the £2 million is to be allocated, but the Long Range Computing Research Policy Panel, in a report published as an appendix to that of the Computing Science Committee, recommends that £1.15 million a year should be spent on research into long range computing and that £300,000 of this be allocated to the purchase of capital equipment.

To reach the manpower targets set by the committee it is suggested that 165 advanced course studentships and 90 research studentships be allocated in 1972. In 1975 the recommendations are that there should be 500 advanced course studentships and 210 research studentships—which will produce the predicted 210 qualifiers in 1978. In 1971 the SRC allocated 139 advanced course studentships and 80 research studentships.

The committee hopes that this growth of 30 per cent over the next five years will be financed jointly by the SRC and the universities, but it says that while universities are adjusting to the need for expansion in computer science, special consideration should be given to the appointment of research assistants for longer than the usual three years.

One of the chief concerns of the committee is that an expansion in the application of computers will not be matched by a corresponding increase in the support of theoretical studies and it cites several fields of research which need special attention. These are analysis of programming errors, computer networks and communications, computer systems evaluation and reliability studies, data processing and large data

bases, general principles of pattern recognition, machine intelligence, micro-programming and related activities, parallelism in hardware and software, physical and logical structure of computers, programming methodology, software engineering, terminals and peripherals and theory of computation.

The committee emphasizes that its shopping list is not exclusive, and that any promising and apt proposal will be considered. It justifies its recommendation for a rapid increase in postgraduate courses by saying that the present surplus of trained computer personnel is short lived.

On courses, the committee says that syllabuses should be more closely oriented to the needs of the industrial and commercial computer user, and it suggests that there should be a choice of options, which should include in particular numerical analysis, available to the student. The committee also says that arts graduates should be encouraged to apply for SRC studentships and that lecturers for courses should be drawn from a wide field and should, in particular, include people from industry.

PLANT BREEDING

Genes for All Seasons

Two new strains of high yield, semi-dwarf, winter wheat have emerged from the Agricultural Research Council's Plant Breeding Institute in Cambridge. They incorporate the dwarfing gene from Norin 10, the Japanese dwarf strain on which Dr N. Borlaug's green revolution was based. In Britain, the new varieties go to the National Trials this year, and Dr F. G. H. Lupton, who heads the cereals division at Cambridge, estimates that they will produce at least 10 per cent more than their closest competitor. One snag is that the increase in yield is associated with a lower relative protein content, but in Britain and other developed countries, where the population is not dependent on cereals for protein, this is an economic rather than a nutritional problem.

Wheat with a high protein content is necessary for making the long-keeping bread which the British consumer now apparently prefers, which means that 60 per cent of the flour in a British loaf is imported from Canada or Australia. Dr Lupton is, however, hoping to increase the protein content of the semi-dwarf wheat so that the proportion of British flour used in bread-making can be increased.

So far, the new varieties seem to show good resistance to the yellow rust fungus. The institute now favours breeding for a degree of resistance to disease in general, rather than for complete specific resistance to known races of fungus. Its view is that varieties with

specific resistance will sooner or later fall victim to new specific variants of yellow rust. Justification for this view has arisen this year with a serious attack of rust on the popular variety Joss Cambier, which until last year was resistant to all known races.

In specific resistance, plant cells attacked by fungus die and the pathogen is then unable to multiply. General resistance, like that of some of the older varieties of wheat, is genetically much more complicated and affords only limited protection. But unless circumstances are exceptionally favourable for the pathogen, as during this appalling summer, the infection will be too limited to affect yield seriously. In the meantime, the lesson of Joss Cambier is that farmers should grow several varieties of wheat.

TELEVISION

Cable TV At Last

from a Correspondent

BRITAIN has an extensive network of cable and wires capable of carrying a great many television programmes. This week, virtually for the first time,

Second Nature?

Enquiry, Harrow County School for Boys' annual science journal, is as professional a job as ever. Larger than in the past, and leading off with articles by such luminaries as Sir Peter Medawar, one-time director of the National Institute for Medical Research, and Dr D. F. Shaw, fellow in physics at Keble College, Oxford, the journal presents its accustomed mixture of sixth form research, work by old boys now at university, accounts of visits to research establishments during the year, together with book and equipment reviews.

Among the original work contributed by the sixth forms are studies of the comparative toxicity of biological and conventional detergents and a continuing investigation into eye size mutants of *Drosophila*. Impeccably produced, *Enquiry* reaches a standard of presentation and content that any school could envy and its only tragedy is that this may be the last year the journal will appear as the Harrow County School's work. The school is to be converted into a high school, teaching only to O-level, if the London Borough of Harrow's plans for comprehensive reorganization in the area go through (see *Nature*, 235, 298; 1972), as now seems likely.

the unique capacity of this network to carry locally originated television was put to use. Greenwich Cablevision Limited began what is to be a year's experiment in providing sections of Woolwich and Plumstead with Britain's first local television channel. The programmes will go out for an hour in the evening and for several hours on Saturday, with repeats on weekday mornings and in the small hours of Sunday morning. They can be received by those of Greenwich Cablevision's 14,000 subscribers who have dual-standard television sets capable of fine tuning.

Considering the enormous potential of cable television, the first ever means of diversifying the choice of television available to the viewer and eventually as a broadband communications line between any television set and the outside world, it was dismaying to hear the apologies with which the Greenwich experiment was introduced. One of the founding fathers, Mr Timothy Dudman, began by saying that the new venture "would never aspire" to compete with national network programmes. The Minister for Posts and Telecommunications, Sir John Eden, emphasized that he was inaugurating a "purely local TV service". He defined the purpose of the experiment (for which his predecessor, Mr Christopher Chataway, had the daring to grant a licence last April) as that of finding out whether "local programming would meet a permanent need of the viewers".

This defensiveness is a measure of the threat that cable television presents to the national broadcasting authority. For if, as Sir John promised, he is to license five other similar experiments, all to be conducted before the great review of broadcasting policy that must come before 1976, then there is nothing to stop entrepreneurs, educators and engineers from dreaming of linking their local systems into national networks. These would not have to be community-based. They could link universities, factories, hospitals or film libraries showing old movies for a fee. The viability of such possibilities will not be measured by the success or failure of television based on local news and features.

Nevertheless, community television is an important new use of the 35 video channels that the cable makes available. The Greenwich experiment is to be determinedly community-oriented. Even so, it cannot help but challenge the elitism of the BBC and the ITA. The opening show revealed a television camera being wheeled down an ordinary street and the expensive equipment being put literally into the hands, some small, some old, of the people. The publicity being given out in the area tells residents that "if you have something to say on local TV, we can

give you that opportunity whether you are a subscriber or not". The price of the service is undeniably low—£0.15 a week—a far cry from the \$5.00 which American subscribers pay for cable television. The Greenwich service includes, incidentally, more than community television; it offers the two BBC channels, three of ITV (London, Southern and Anglia) and VHF radio as well. Greenwich Cablevision is prevented from using any advertising. It hopes to pay for the experiment by attracting several thousand new subscribers.

WORLD HEALTH ORGANIZATION

Genetic Diseases

AN increase in medical genetics services is recommended by a World Health Organization scientific group in a report published recently (*Genetic Disorders: Prevention, Treatment, and Rehabilitation*, WHO Technical Report Series, No. 497).

Interest in medical genetics, according to the WHO report, has increased with the decrease in morbidity and mortality attributable to infection, parasitic infestation and malnutrition and the increase in morbidity and mortality attributable to genetic factors which have resulted from slow changes in the gene frequencies.

CHANNEL TUNNEL

Decision by Degrees

THE tunnel beneath the English Channel (or La Manche as others call it) still looks a sound investment. The results of the latest studies are now with the British and French governments, and statements are expected shortly outlining the chief conclusions and approving the final phase of study and exploration. Details of the report remain a well guarded secret, but informed sources suggest that the financial prospects look good in spite of the rocketing cost of the project and in spite of reported differences between the French and British assessments of profitability. The French estimates are apparently more optimistic than the British.

The cost—a mere £130 million in 1960—must now be not far short of £350 million, but the Channel Tunnel Investment Company and the Société Française du Tunnel sous la Manche are both keen to continue with the project. The two consortia include an impressive array of 20 hard-headed construction companies and merchant banks, with British and French Railways thrown in. Having put the better part of £1 million into the studies so far, they are naturally keen for a decision to push ahead.

Both governments have retained the right to invest in the project and approval

There is no reliable information available at present, according to the report, on the prevalence of genetic diseases or on the effects that treatment and prevention will have on the incidence of such diseases. The aim of the report is to present information on genetic diseases so that community efforts to counter them are more effective.

Medical genetics services should be available at each medical school and the head of the counselling clinic should be trained in medicine as well as human genetics. In particular, the report says that the existence of the clinic and its role should be made widely known to all doctors, and that there should be close collaboration between the clinic and the general practitioners.

The WHO scientists agree that there is a need for more research into prenatal diagnosis to ensure that the offspring of genetically high risk couples are unaffected. The report says that a considerable increase in obstetric and laboratory facilities is needed for this to happen.

Further recommendations are that children born after amniocentesis should be studied to assess the long term effects of this procedure and that further research be carried out to develop new and improved techniques for the preclinical detection of individuals with genetic diseases.

for the first construction stages could be given in the autumn of 1973. On that plan, the tunnel might be in full operation by 1978, although 1980 is probably a more realistic date.

The channel remains an attractive proposition because the growth of cross-channel traffic is now expected to be faster than previously thought. Even on the lower estimates of the amount of traffic the tunnel would generate and take from ferries, ships and aircraft, the project is held to be an attractive investment. The original idea, in 1960, was that the consortia would build the tunnel and lease it to the governments but this idea collapsed because of international legal and financial restrictions. Then, however, the figures suggested that the economic rent would be £6 million a year against an estimated income in the first year of £13 million. If the costs have inflated, so have the returns.

If the governments are as enthusiastic as the consortia—and the governments have to consider sociological and environmental effects as well as profitability—a decision next year to go ahead would involve about £20 million expenditure by 1975 on the first construction stages. If the governments are still uncertain, they could build in another decision stage in 1975, when, if any unexpected technical difficulties appear, the project could be abandoned at a reasonable cost.

NEW WORLD

Jostling for Space in Orbit

from a Correspondent

COMMUNICATIONS satellites are an economic form of message transmission when they connect points about 1,000 miles or more apart. If they are used to connect widely scattered places as well, their advantage over land lines and microwave chains is even more conspicuous. That considered, it is astonishing that the United States will not have domestic satellite links until 1974 at the earliest. A decade will have elapsed since the Syncom II brought the Tokyo Olympics to the world's television screens and nine years since Early Bird introduced commercial satellite communications across the Atlantic.

Why the delay in using satellites at home? The short answer is the Federal Communications Commission. The FCC, one of the hard-worked, little-praised regulatory agencies which look after essential American industries on Congress's behalf, has been trying since 1966 to design a policy for the introduction of satellites into the domestic communications scene. Its dithering has been an object lesson in the problems of fitting new technology into the bureaucratic structure of the past. There have been three main contenders for the right to own and operate American domestic satellites. None could proceed without the FCC's permission.

One was the American Telephone and Telegraph Company. AT&T is the nearest American equivalent of the British Post Office—it is the big communications power in the land—but its monopoly is nowhere near as great. AT&T runs the bulk of American local telephone services, all of the inter-state trunk telephone services, the Department of Defense's private communications network and the distribution of national television programmes. AT&T sees satellites, just as the Post Office does, as one of the techniques which it needs if it is to have the versatility which a national telephone service demands.

But in 1964 Congress decided that AT&T must keep its hands off satellites, at least as far as they were used in international communications, which was all anyone was talking about at that time. That year, Congress created the Communications Satellite Corporation (Comsat) to run the American end of the international system then being formed. Comsat was hobbled by having AT&T on its board (AT&T having become, understandably, a strong advocate of undersea telephone cables). Since 1966, Comsat has been pressing the

FCC to give it a monopoly on domestic satellite services.

The third possible way to deal with domestic satellites was to let satellite users — the television networks and the news distribution agencies, for example—put up their own. In fact, it was the American Broadcasting Company, one of the television networks, which initiated the whole controversy back in early 1966 by asking the FCC if it could not put up a satellite for television alone. The networks felt they could save about half the \$70 million a year they were paying AT&T for landline transmission if they could use satellites instead.

Weighing the economic and technical considerations of the various proposals nearly drove the FCC out of its collective mind. Prodded by an influential Task Force on Communications Technology which reported to President Lyndon Johnson in 1968, the FCC was about to give the prize to Comsat—at least temporarily—but it hovered too long. In January 1970, President Nixon's Office of Telecommunications Policy gave its opinion. Let the sky be open to all comers, the OTP said in a memo to the FCC. Let all qualified applicants who wish invest in a satellite (which would be launched for several million dollars by the National Aeronautics and Space Administration) and let the market place decide which shall win the American public's satellite business.

But the FCC, even with Republicans outnumbering the Democrats among the commissioners by four to three, is an independent agency, answerable only to Congress and not to the President. Last week it announced its decision and it was vintage FCC—full of temporizing and splitting the difference.

Nevertheless, it will allow domestic satellite projects to proceed at last. Competition in the American sky is most certainly to be allowed. But both AT&T and Comsat will be prevented from entering freely into it. All applicants who can show that they have the financial and technical ability to run a satellite system and that their proposed service would fill a demonstrable need will be allowed to try their luck. But AT&T, for three years, can only use its satellites within the continental United States only for telephone services. In other words, it cannot use the revenues it enjoys from its near-monopoly on telephone services in order to compete against other companies offering new kinds of communications facilities—

data transmission, television distribution and private line services. And Comsat, for its part, will not be allowed to put up the two distinct systems it had in mind—one in conjunction with AT&T and the other serving customers like the television networks and news services directly. These restrictions on the two companies most likely to initiate domestic satellite service so offended three of the Republicans on the FCC, including the Chairman, Mr Dean Burch, that they dissented from the decision. Nevertheless it stands.

Final applications are due at the FCC this month. Among companies other than AT&T and Comsat which have placed satellite plans before the FCC are the Western Union Telegraph Company, the MCI Lockheed Satellite Corporation (a joint company formed by a new inter-state specialized microwave carrier and Lockheed), Hughes Aircraft Corporation and Fairchild Industries. The delay between the FCC's approval and actual launching is expected to be between 18 and 24 months. Ironically, the economics prospect of most of the new system is now jeopardized by the growing doubts of the television networks. They wonder if satellite transmission would be cheaper for them after all; perhaps, they are thinking, they should stick with proven, reliable AT&T.

NOBEL PRIZE

Professional Ethics

DR ORESTE PICCIONI, of the University of California at San Diego, is suing the 1959 Nobel prizewinners in physics, claiming that they stole the idea of the prizewinning work from him. Dr Piccioni alleges that Dr Emilio Segre and Dr Owen Chamberlain of the University of California's Lawrence Laboratory took his idea of using double magnetic lenses to focus the beam of the Bevatron accelerator at Berkeley, a development which played a critical part in the experiment to prove the existence of antimatter in 1955. Dr Piccioni claims that he made a "precise pact of collaboration" with the two physicists that they do the experiment but that he should be credited with the idea. Subsequently, Piccioni claims, he was manoeuvred out of the project and his name was not included on the paper announcing the discovery.

To rectify the situation Dr Piccioni is suing for \$125,000 damages, plus public admission by the defendants of his role in the experiment.

NEWS AND VIEWS

Mistakes in Ageing Fibroblasts

It is interesting to examine the historical aspects of the work which is reported by Holliday and Tarrant on page 26 of this issue of *Nature*, for it is now some nine years since Orgel suggested that an "error catastrophe" might be responsible for the senescence of cultures and something like eleven years since a relationship between the limited life span of cultured human diploid fibroblasts and ageing was proposed. Why, it might be wondered, has it taken so long for anybody to relate these two ideas?

The suggestion that senescence might result from an accumulation of defective protein molecules has not yet received a great deal of attention, for although there is some evidence that death occurs in certain mutant strains of fungi when large amounts of defective proteins are synthesized, this is the only evidence which suggests that an error catastrophe is possible. An initially low level of mistranslation with only a few cellular proteins carrying amino-acid substitutions may become self-perpetuating so that the defective proteins contribute to the synthesis of larger amounts of altered proteins. Evidence which might relate the idea to what is generally accepted as ageing has, however, not yet been forthcoming.

Hayflick's work, on the other hand, after an initial sceptical reception, is now supported by a considerable amount of work from several laboratories. Various cell strains of both mammalian and avian origin all demonstrate the same sort of limited, *in vitro*, life span and, although so far there is no reason to believe that the cause of ageing in these cells is the same as that which causes ageing of the whole organism, it would seem that the two must be related in some way. The number of times diploid skin fibroblasts can be cultured depends very largely on the age of the individual from whom the cells derive, so that although not everybody believes that this limited life span is an artefact, the phenomenon of ageing in fibroblasts now seems to be generally accepted.

Much of the work to date has been directed towards characterizing biochemical and physiological changes which arise in senescent fibroblasts and it is now known that changes in the proportions of various enzymes occur and that there are alterations in the ratio of protein to DNA. None of the observations, however, clarifies the basis of ageing in these cells and, although it has been tentatively suggested that the event is genetic and of a quasi-differentiated nature, the evidence for this is really very poor. Holliday and Tarrant now believe that Orgel's idea might be correct and they suggest that the altered proteins which they detect in senescent fibroblasts derive from an error catastrophe.

A strain of human embryonic fibroblasts, similar to the one originally described by Hayflick, seems to contain an increasing proportion of at least two altered enzymes—glucose-6-phosphate dehydrogenase (G6PD) and 6-phosphogluconate dehydrogenase (6PGD)—as the cells grow older. The evidence for the suggestion that these enzymes contain amino-acid substitutions is based on the fact that enzyme extracted from old cells is considerably more thermolabile than that from young cells, with some 25 per cent of the enzyme from senescent cells breaking

down rapidly when extracts are heated. Numerous studies have already shown that an association exists between alterations in the amino-acid sequence of a protein and increasing susceptibility to breakdown, but Holliday and Tarrant also find that such alterations are associated with changes in substrate specificity. G6PD is normally capable of catalysing dehydrogenation of the deoxy-form of substrate at about 5 per cent the rate of that with glucose-6-phosphate, but in ageing cells considerably more deoxy-glucose-6-phosphate can be utilized. The fraction of enzyme which demonstrates this change in substrate specificity is the same as that which shows increased thermolability so that the proteins which contain amino-acid substitutions would seem to have altered affinities as well as increased susceptibility to temperature.

It is argued that all the proteins of senescent cells will contain the same number of errors, but it is a pity that the two enzymes which came under Holliday and Tarrant's scrutiny were not from widely different pathways. The amounts of both G6PD and 6PGD which are thermolabile and therefore altered are approximately the same and increase in a similar manner in ageing cells, but one wonders whether this is sufficient evidence to suppose that all the proteins of a cell are altered.

The results of experiments with the drug 5-fluorouracil seem, however, to provide the most convincing evidence that senescence and errors in protein synthesis are associated. Because this drug finds its way into all species of RNA subsequent mistranslation of all cellular proteins will occur, and growing fibroblasts in medium containing this drug does seem to induce premature senescence. 5-Fluorouracil seems to have no immediate effect on the cells, but after they have passed through several rounds of division altered, thermolabile G6PD and 6PGD can be detected and this is again associated with a loss of growth potential. The drug induces changes similar to those found in naturally senescent cultures, and Holliday and Tarrant think that it is not unreasonable to suppose that ageing arises as the result of mistranslation.

On the face of it these results would seem to provide strong evidence for Orgel's error theory of ageing but, as seems to be the case with any essentially simple idea, the ramifications may be much more complex.

Evidence provided by work published during the past few years indicates that increased rates of mutation are found where cells either contain altered polymerases or are grown under conditions which might induce alterations in enzymes concerned with DNA replication. Holliday points out that errors in such proteins could be induced during ageing and, although somatic mutation theories of ageing have now largely fallen into disrepute, lethal and other types of mutation may be responsible for impairing cell growth either directly or indirectly. To complete the cycle of events, it is also possible that mutations may arise which will increase the error frequency still further.

The real point at issue, however, is whether an increasing proportion of error containing proteins is the primary cause of ageing in these cells. Although Holliday and Tarrant's work suggests strongly that there is some rela-

tionship between the two it must be regarded as an opening gambit rather than the final word on the subject. Before the sceptical fraternity will be convinced that either an error catastrophe, or mutations arising as a result of such errors, are responsible for ageing, several points will need to be clarified. In the first place, it is important to know whether these mammalian cells have the same propensity for breaking down altered proteins as bacteria and why altered proteins are not constantly removed from the cells. Although Holliday and Tarrant consider that mammalian cells may be much less tolerant of error containing proteins than either fungi or bacteria, it would be interesting to know whether or not the accumulation of altered protein is not merely the result of the fact that the machinery responsible for "cleaning up" cellular proteins is in some way defective. Altered protein may not be the cause but the result of ageing.

Again, if it is accepted that mutational events play a significant part in the ageing of mammalian cells do these cells show more resistance in this respect than either bacteria or fungi? Medvedev (*Nature*, **237**, 453; 1972) has recently pointed out that gene duplication may serve to protect an organism from just this sort of mutational event so that, in theory at least, mammalian cells could be considered to be less susceptible to ageing than prokaryotic cells. Is this ridiculous?

It is not yet possible to find all the answers in an error theory, but must biologists really wait another decade for the next clue?—From our Cell Physiology Correspondent.

Intensity of Solar Neutrinos

It has long been realized that stars are important nuclear physics laboratories. Within them nuclei are cooking under conditions which cannot be reproduced on Earth. From a sufficiently detailed understanding of a star's structure, therefore, important inferences can be drawn about rates of thermonuclear reactions.

Because of the Sun's proximity more is known about it than any other star, so that any theory of stellar structure ought to be able to reproduce its major properties. Mathematically, the object is to solve the equations describing the evolution of a solar model from some initial conditions over the Sun's lifetime. The resulting solution must then be compared with what is known about the real Sun. Unfortunately it is not known exactly how old the Sun is, nor what it was like at birth (the initial conditions), though bounds can be put on the possibilities; and until recently the only information available about its state, apart from its mass, came from observations of its surface only.

With so much freedom in the choice of input parameters (not to mention the latitude allowed in the equations themselves arising out of uncertainties in the physics) and with so little to predict, it is not altogether surprising that solar models have been successfully constructed.

Now all this has come to an end. In 1968, after years of painstaking labour, R. Davis, jun., and his collaborators were able to set a useful upper bound to the flux of neutrinos coming from the Sun. The neutrinos are produced as byproducts of the nuclear reactions occurring near the centre of the Sun; a knowledge of the rate at

which they are produced more or less doubles the data that stellar evolution theory must predict. It came as a great surprise to the experts to discover that no matter how hard they tried they could not build a model with a neutrino flux anywhere near as low as Davis's upper bound.

For some astrophysicists, this discrepancy between theory and observation was extremely significant; perhaps most of all for Professor William A. Fowler of the California Institute of Technology who realized that the physics of the solar interior needed to be reworked. No startling revelation has emerged from the physics, only minor quantitative modifications; but the effort was worthwhile because there is now greater assurance that nothing is seriously wrong with previous calculations. Curiously, the resulting improvements to the solar models have generally tended to depress the neutrino flux; there still remains a considerable gap between the theoretical predictions and the latest measurements.

It is not entirely inconceivable that the accumulation of many small errors might have conspired to produce the still large discrepancy, but serious physicists must continue to entertain the possibility that there is still a particular qualitative error to be found. It is worth pointing out too that because some of the procedures which are used for constructing models of other stars are calibrated against models of the Sun, the theory of stellar evolution must be viewed with considerable caution while the solar neutrino problem remains unsolved.

In the past few years many astrophysicists have expended a great deal of effort discussing what phenomenon might have been left out of the theory. Solar models have been endowed with high magnetic fields or rapidly rotating cores, for example, but all to no avail. Most of the ideas which would successfully pull down the neutrino flux to a low enough value stand little hope of being tested in the foreseeable future, and it is possible that physicists will one day be forced to accept, for example, the suggestion that the neutrino decays in a time comparable to that taken to traverse the 10^8 km from the Sun to Earth. The two suggestions made by Professor Fowler in his article on page 24 of this issue of *Nature*, however, do not fall into this category.

Fowler's suggestion that a narrow resonance exists in ^7Be is unlikely but not impossible, and certainly lends itself immediately to experimental refutation. It could effectively prevent the production of ^7Be in the Sun so that only neutrinos from the $^1\text{H}(p,e^+\nu)^2\text{H}$ reaction would be produced. The predicted flux would then be only about half of Davis's current upper bound. Fowler's second suggestion, that the centre of the Sun has recently been mixed, is one which has astronomical consequences other than a reduction in the neutrino flux and so in principle is open to observational checks. Indeed, Öpik argued twenty years ago that intermittent mixing of the Sun is the cause of geological ice ages. Sudden mixing influences the neutrino flux principally by inhibiting the production of ^8B . The contributions from the $(p,e^+\nu)$ reaction and the electron capture by ^7Be are only marginally reduced, leaving the total flux just at Davis's limit.

There are plausible physical reasons for believing that the central regions of the Sun are occasionally mixed, but even if mixing has occurred recently it may not be long before further refinements in measurement indicate that the flux is almost non-existent.—D. O. G.

Strand Switching during SV40 Transcription

THE two DNA tumour viruses, simian virus 40 and polyoma virus, are small and simple and have thus attracted the attention of tumour virologists intent upon understanding the viral transformation of animal cells from the malignant to the non-malignant state. The genomes of SV40 and polyoma virus are double stranded, covalently circular DNA molecules, with molecular weights of about 3×10^6 and are capable of coding only a mere 200,000 Daltons of polypeptide; in other words, these genomes almost certainly comprise less than ten and perhaps as few as six genes, one or more of which presumably specify the protein(s) responsible for maintaining cells in a transformed state.

The ultimate goal, therefore, of many tumour virologists is to identify and characterize this gene and its products, and, given the sophisticated technology of years, seemed entirely feasible. Nevertheless, progress towards it has been painfully slow. Experiments carried out recently in three American laboratories are certain, however, greatly to revitalize flagging enthusiasm for these viruses.

The three groups in question—Khoury and Martin of the National Institutes of Health, who have just published the first of their new results in *Nature New Biology* (238, 4; 1972), Lindstrom and Dulbecco of the Salk Institute, who reported their work in the *Proceedings of the National Academy of Sciences* in June (69, 1517; 1972), and Sambrook, Sharp and Keller of Cold Spring Harbor Laboratories, whose results have yet to appear in print—have all shown that as SV40 replicates in permissive cells stretches of both of the two strands of the viral DNA are transcribed. In short, transcription of SV40 DNA *in vivo* involves strand switching, a device hitherto known to occur only during the replication of some DNA phages with much larger genomes.

Since the late nineteen-sixties many groups, including the three which have made this surprising observation, have used various nucleic acid hybridization procedures in efforts to measure the extent and patterns of transcription of SV40 DNA during its replication and in transformed cells. And in spite of the obvious and formidable technical difficulties it has been shown that as this virus replicates between a third and a half of the genome is transcribed throughout the replication cycle into so-called early RNA while the remainder of the genome is transcribed into so-called late RNA only after the initiation of replication of the viral DNA.

What happens in transformed cells is more obscure and controversial; depending on the hybridization techniques used and the particular transformed cell line studied it has been variously estimated that transcription involves from much less than half to the complete genome. There is, however, general agreement that the viral RNA in transformed cells includes most if not all of the sequences present in the early RNA of lytic infections.

To gain further insight into the transcription of SV40 DNA first in permissive cells and subsequently in transformed cells, the three groups have exploited Westphal's (1970) observation that *in vitro* *Escherichia coli* DNA dependent RNA polymerase transcribes superhelical

SV40 DNA asymmetrically to yield an RNA molecule complementary to only one of the two DNA strands. This RNA made *in vitro* can be obtained in comparatively large amounts and can be labelled to high specific activities. It has now been used to effect the isolation of the separated strands of SV40 DNA and, in conjunction with appropriate hybridization procedures, the three groups have used this RNA to sort out which DNA strands serve as template for the synthesis of early and late RNA made *in vivo*.

The groups all find that early RNA isolated from infected permissive cells before the onset of viral DNA replication is transcribed from the strand that is used at template by *E. coli* RNA polymerase *in vitro*. By contrast late RNA, which cannot be detected at early times during infection, is transcribed off the other SV40 strand, that which is never used by the *E. coli* polymerase. In short the appearance of these late RNA sequences depends on switching the strand which is being transcribed.

These findings, which are a major step forward in understanding of the transcription of SV40, are for several reasons most unexpected. For one thing, in many quarters it had been assumed that the transcription of SV40 DNA *in vitro* by the *E. coli* enzyme, a highly asymmetric reaction, faithfully reflected the situation *in vivo*, but in the event that reaction has proved to be a useful red herring. For another thing, the SV40 genome is very much smaller than the genomes of those DNA phages, the transcription of which involves similar strand switching, and it had been generally assumed that such a simple virus might well regulate its affairs without resorting to the complexity of transcriptional strand switching. The fact that the transcription of the replicative form DNA of phage ϕ X174, which is almost exactly the same size as SV40 DNA, is totally asymmetric bolstered this assumption.

Experimental observations, however, have overtaken prejudice and raise a host of intriguing questions. First, how is this switch from early to late transcription, which presumably demands the existence in SV40 DNA of two promoter sites at which transcription begins and two terminators, effected? Why is the late promoter inactive until the replication of the viral DNA begins or if it is active at early times how is the late RNA specifically degraded? And what happens when the SV40 DNA is covalently integrated into the DNA of a transformed cell?

It is known that viral RNA in transformed cells includes sequences also present in the early RNA made during virus replication in permissive cells. Does this mean that integration involves a break in the SV40 DNA molecule somewhere in the late genes and the consequent synthesis of RNA molecules in which early RNA sequences are linked by a stretch of anti-late viral RNA to host RNA sequences? These and many more questions still have to be answered, but that they are now even being asked, which was not the case only a few months ago, gives some indication of the importance of the discovery of strand switching during the transcription of the SV40 genome.—From our Cell Biology Correspondent.

RIBOSOMES

Protein Gene Expression

from our Cell Biology Correspondent

It is hardly surprising that cells require complex machinery in order to synthesize proteins, which are after all large, specific molecules, but whether a decade or so ago many biochemists anticipated the extreme heterogeneity of the structural proteins of the ribosome may be doubted. The fact is, however, that in a 70S *Escherichia coli* ribosome there are some fifty-odd species of structural proteins and such heterogeneity poses several intriguing questions. Why are so many different sorts of proteins required and what do they do? And how are the corresponding structural genes arranged in the *E. coli* genome such that their expression can be coordinated and regulated? That these genes are expressed in a coordinate manner is indisputable, of course, for in bacteria the number of ribosomes per cell is directly proportional to the growth rate.

The obvious first thought is that the structural genes of ribosomal proteins might be organized into operons and the clustering of at least some of these genes is at least consistent with that idea. But the experimental approaches involving the isolation of deletion and nonsense mutations of structural genes and mutations of operators and promoters, which have been used to characterize conventional operons, are clearly inappropriate and unrewarding when it comes to analysing the organization and regulation of ribosomal protein genes because drastic mutational changes in these genes are bound to be lethal. Clearly, the problem demands of bacterial geneticists the sort of ingenuity which Nomura and Engbaek (*Proc. US Nat. Acad. Sci.*, **69**, 1526; 1972) encouragingly bring to bear upon it.

Nomura and Engbaek have exploited stable, partial diploid strains of *E. coli* which are heterozygous for the region of the *E. coli* chromosome which includes the spectinomycin, streptomycin, erythromycin and fusidic acid loci at which there can be sensitive or resistant alleles. These loci specify respectively the S5 and S12 proteins of 30S ribosomal subunits, a protein of the 50S subunits and the elongation factor G. The particular strain they have used carries on the chromosome the markers *argG*⁻, *ery*^s, *spc*^s, *str*^s, *fus*^s, *aroB*⁻, *malT*⁻ in that order, and an episome carries the markers *argG*⁺, *ery*^r, *spc*^r, *str*^r, *fus*^r, *aroB*⁺, *malT*⁺. Because the sensitivity alleles are dominant over those conferring resistance the cells have a sensitive phenotype to all four antibiotics.

To determine whether these four loci, which specify ribosomal proteins and an elongation factor, are part of a single

transcriptional unit and therefore expressed coordinately, Nomura and Engbaek took advantage of that most useful phage, Mu, which can lyso-genize an *E. coli* cell by integrating at random throughout the bacterial chromosome. If phage Mu integrates in the middle of an operon, the expression of those cistrons distal to the promoter and the Mu genome is blocked but the cistrons lying between the promoter and the prophage are expressed normally.

Assuming that the four loci in question are part of one transcriptional unit, if Mu integrates in one of them those receiving or distal to the prophage will be inactivated whereas those proximal to the prophage will be unaffected. And because of the resistance alleles carried in the cell by the episome as soon as one or more dominant chromosomal alleles are inactivated the cell will acquire a resistant phenotype. If each of the four loci is transcribed as a discrete unit, however, each, to be inactivated, must receive a Mu genome.

Having predicted the various possible outcomes, Nomura and Engbaek infected their partial diploid cells with Mu phage and analysed the pattern of drug resistance in the infected cultures. All their results are consistent with just one of the predictions which says that the four loci are indeed part of a single transcriptional unit or operon in which the "promoter" is to the left of the four loci *ery*^{s/r}, *spc*^{s/r}, *str*^{s/r}, *fus*^{s/r}. The behaviour of spontaneous resistant mutants is also consistent with this model; for example, more than half the spontaneous erythromycin resistant mutants are also resistant to the other three drugs and

may well therefore carry polar nonsense mutations. Furthermore, the ribosomes from Mu induced mutants resistant to all four drugs behave, as expected, in a cell free system making protein.

These data clearly indicate that these four loci form part of some sort of transcriptional unit, and Nomura and Engbaek, who comment that a Belgian group has also found that the *spc*^{s/r}, *str*^{s/r} loci are part of transcriptional unit, suggest that many if not all the structural genes of ribosomal proteins are probably in this one massive "operon". Whether the transcription of this ribosomal protein operon is regulated in the way that the transcription of conventional operons is regulated is but one of the fascinating questions which, by further exploitation of partial diploid strains, may soon be answered.

ARTIFICIAL INTELLIGENCE

Understanding Program

from our

Experimental Psychology Correspondent

"UNDERSTANDING Natural Language" is not a bad title for a scientific article, but when the article in question describes a computer program that actually performs this feat, it is even more impressive. The editor of *Cognitive Psychology* has made a sound decision in devoting a whole issue of the journal (**3**, 1; 1972) to an article by T. Winograd of the Artificial Intelligence Laboratory at Massachusetts Institute of Technology. All the indications are that work in artificial intelligence is becoming extremely important for psychology.

A Structure for Thermolysin

A SET of three articles in next Wednesday's *Nature New Biology* (July 12), from the laboratories of Neurath in Seattle and Matthews at the University of Oregon, report the sequence and 2.3 Å-resolution X-ray structure of the proteolytic enzyme, thermolysin. Thermolysin is a bacterial metallo-enzyme of molecular weight 35,000, which contains both zinc and calcium, and is unrelated to the serine and the thiol proteases. The sequence shows some polarization of charged residues. It is not related to that of carboxypeptidase, a protease that it resembles in molecular weight, composition and the presence of zinc.

The X-ray structure reveals that the molecule has the form of two globular entities, separated by a deep cleft. Within this cleft is the zinc atom, which is known to be necessary for enzymatic activity, but can be extracted by a chelating agent without destruction of the crystal. There are several internal helical segments. The zinc ligands are

identified as two histidines and a glutamic acid; this is the same combination as in carboxypeptidase, which is in other structural respects quite different.

Other active site residues, which may be involved in catalysis, are a buried glutamate, an aspartate and an arginine associated in an ion pair, a tyrosine and a histidine bound by a salt link to another aspartate. The presence of calcium is a feature that thermolysin does not share with carboxypeptidase. There are four calcium binding sites, two of them joined in a cluster of five carboxylate groups which are partly buried. The other two sites are superficial, and also involve carboxyl groups. These calcium ions are not essential for activity, but are believed to confer thermal stability on the structure.

The structure in the presence of substrates or inhibitors has not yet been determined, so that no direct inferences about the enzymatic mechanism are at this stage possible.

Winograd's research is a milestone in progress towards solving the problem of how knowledge can be represented in such a way as to allow that whole constellation of processes suggested by the term understanding.

Basically Winograd's program will accept input in ordinary English from a teletype, will understand it, carry out commands and answer questions. The subject of discourse is limited to a simple scene containing toy blocks, and the program allows conversation with a notional robot which has an eye to observe the scene and a hand to manipulate it. (In fact the scene in this program is simulated in the computer, although a real robot capable of interacting with real blocks also exists in the same laboratory.) Winograd presents a conversation with his program and then goes on to describe the general theory of language on which the system is based, and the particular sub-programs with which it is implemented.

A sample interchange which he quotes is, Winograd: "Find a block which is taller than the one you are holding and put it into the box." Computer: "By 'it' I assume you mean the block which is taller than the one I am holding." The computer prints this out as a commentary on its interpretation and reports "OK" when the command is completed. Even in this simple example the computer has to deploy a knowledge both of the particular blocks it is dealing with, and of the more general properties of its world; for example, it accepts the interpretation that a block can be put in the box, but would reply (if asked) that the box could not be put in the block.

One of the key features of Winograd's program is that it does not depend merely on syntactic analysis to understand the meaning of sentences. The system contains a series of sub-programs, or procedures which can call each other, and this allows analysis of strings of words to proceed partly by means of grammatical rules, and partly by semantic interpretation. The semantic knowledge of the system is embodied in a formal structure where objects and their properties and relationships are represented.

The goal of analysing a sentence is to interpret it into this formalized structure, because by so doing programs that will undertake logical deductions by computing over this knowledge can be deployed either to test whether a particular interpretation of a string of words is plausible, or to answer a question. Thus there is a continual interchange between syntactic and semantic analysis, until a meaning has been reached. The meaning in this case corresponds to statements in its knowledge structure, and that in turn corresponds to a mental structure of concepts which can be

manipulated by processes of inference.

Though Winograd's program still works in a limited domain (of a world of toy blocks) it is a substantial advance on previous systems. It breaks right away from question-answering programs which spouted back slightly rearranged pieces of text and even from those systems in which questions of certain specified kinds could be accepted and answered by applying a uniform proof procedure to information stored in predicate calculus form. That method, though mathematically elegant, turned out to be vastly inefficient for large quantities of data. Winograd's system is more flexible and more general, as well as allowing easy addi-

tion of new knowledge to the system.

At one level the success of Winograd's program depends upon the availability of high-level programming languages such as PLANNER which was developed at MIT and knowledge and meaning are represented as expressions in this language in a way that allows inferential processes to be carried out. At another level it depends upon the idea of representing knowledge as program procedures, because any piece of knowledge can be related to any other by calling the procedure directly. It is not unreasonable to suppose that human intelligence too depends upon just such flexibility of interrelations between concepts.

Hard X-Ray Flare in Cygnus X-1

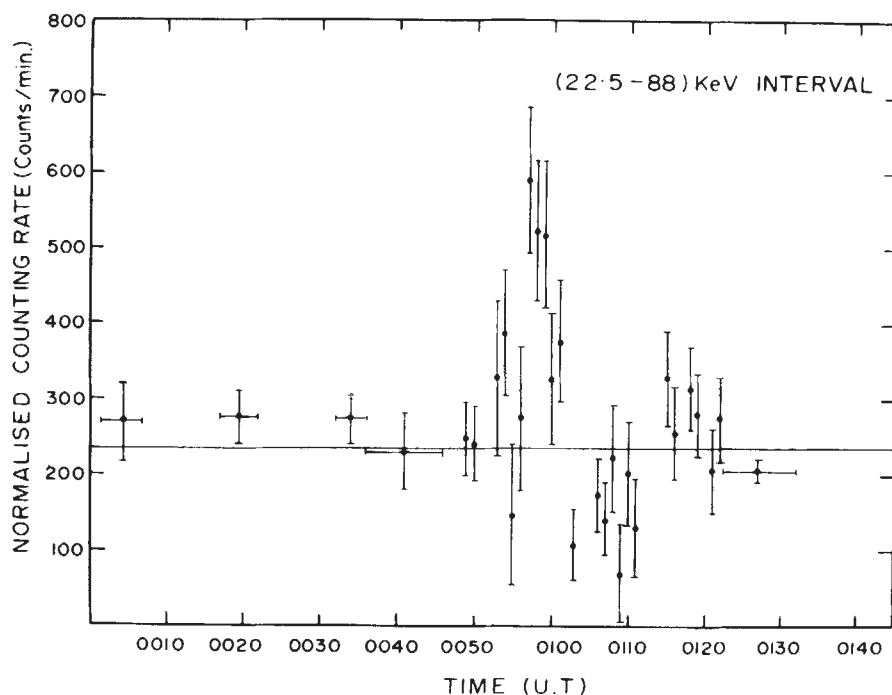
FURTHER investigations of balloon flight data obtained by P. C. Agrawal and others (*Nature Physical Science*, next Monday, July 10) have revealed that Cyg X-1 is subject to flaring at hard X-ray frequencies, and that this flaring is comparable to the hard X-ray flaring recently observed in Sco X-1.

The data which have revealed this activity were obtained last year, and first results from the balloon flight were reported by the same team in *Nature* (232, 38; 1971). At that time, they were chiefly concerned with time variations of the flux from Cyg X-1 over periods of several minutes. But more recently rocket and satellite experiments have pointed to the particular peculiarities of Cyg X-1 which suggest that more rapid variations in the flux are likely. Agrawal *et al.* have therefore re-examined their data using the best resolution which it allows—a time spacing of one minute. The result is

that they have discovered a flare which occurred on April 5, 1971, in the energy range 22.5 to 88 keV (see figure).

In the space of a minute, the flux increased from about 253 counts/min to about 589 counts/min, remaining high for two minutes before declining over a further two minutes to something like its "normal" value. The three highest values recorded are each more than 3σ above the mean counting rate, and the probability that three consecutive observations like this could occur by chance is less than 1 in 10^7 .

According to Agrawal *et al.*, the spectrum of the source remained unchanged during the flare, although its intensity increased by a factor of ~ 2.3 . Overall, this behaviour is strikingly similar to that observed in Sco X-1 (see, for example, Hudson *et al.*, *Astrophys. J. Lett.*, **162**, L115; 1970), although too much emphasis should not be placed on a single observation.



BIOENGINEERING

Catch-all or Cornucopia?

from a Correspondent

THE two faces of bioengineering were clearly displayed during the symposium held from June 19 to 21 at the University of Strathclyde, Glasgow.

Half of the programme was devoted to biomechanics—the application of engineering principles and techniques to the study of human bones, joints and tissues. Much of this work is at an early stage, involving experiments on animals or on post-mortem material, but direct measurements of pressures in the spine (using an implanted transducer with radio telemetry) reported by Professor A. Nachemson and Dr G. Elfström (University of Gothenburg) are beginning to give useful information on the treatment of patients with low back pain and on post-operative management after bone surgery.

Dr C. A. McLaurin (Ontario Crippled Children's Centre, Toronto) considered that biomechanics is still an academic discipline of little value to the clinician. He offered two reasons for this failure. First, the human body does not resemble a man-made machine; most machines are designed to suit the engineering principles on which they are based—but the mechanical principles of the body are not yet understood. Second, said Dr McLaurin, the patient management process does not lend itself to systematic method. Doctors have little patience with the promise of biomechanics, because they work in clinics where problems must be analysed and sometimes found in a matter of minutes. He urged that biomechanics should be brought closer to reality by the greater involvement of practising members of the medical and remedial professions.

Professor D. Dowson and Professor V. Wright (University of Leeds) reported that the remarkably effective mechanism for the lubrication of load-bearing joints still defies analysis in engineering terms. Attempts to treat stiff joints by the injection of silicone or other artificial lubricants have not been successful.

A session on artificial organs was dominated by the views of Mr D. Longmore (National Heart Hospital, London) who suggested that 75 per cent of patients with chronic kidney failure die unnecessarily because of lack of renal dialysis facilities—a situation which he described as “murder by neglect”. He thought that artificial kidney treatment would be better provided by mobile units, based on double decker buses, which would visit hospitals.

Mr Longmore dismissed current research towards the replacement of the ailing heart by a mechanical pump as

largely wasted effort, through lack of clear medical leadership and achievable specifications. Acknowledging that heart transplants were at present unacceptable to society he suggested that more attention should be given to other biological treatments; tissue affected by a myocardial infarct might be revascularized by an implant of foetal heart tissue.

The symposium confirmed the view, expressed by several speakers, that bioengineering has not yet made much impact on the clinical scene. The most successful work has been done in biomechanics; excursions into physiology or medical physics have often been unproductive or unconvincing.

OCEANIC FISHES

Biology of Bramids

from our Marine Vertebrate Correspondent

THE bramids are an oceanic family of fishes with a worldwide distribution except for the polar seas. Many species have been described, but a recent revision by G. W. Mead (*Dana Report*, No. 81, 1; 1972) reduces them to eighteen species in six genera. Mead's revision will be welcome to taxonomists in particular, but fisheries biologists will also welcome it because it clears away some of the dead wood of nomenclature in the group.

The reason for the plurality of names in the group is in the main two-fold. First, many of the species are moderately large when full grown, some even reach-

ing a metre in length, and have thus engaged the attention of biologists from the earliest times (the earliest record of a bramid in the literature seems to be the specimen of *Brama brama* described by John Ray in 1686). Second, the young fish are often totally dissimilar to the adults in body shape and other features. A third and complicating factor lies in the relatively few specimens in marine collections.

Mead's revision has been made possible by the use of the vast collections of young fish in the Danish Carlsberg Foundation, made by the research vessel *Dana*, supplemented by museum collections. A most useful additional source has been supplies of juvenile fish taken from the stomachs of tuna and billfishes, predators that seem able to catch the size of fish that nets and hooks miss. Possession of long series of larval and juvenile specimens as well as adults has meant that it has been possible to link known juvenile forms with known adults, allometry notwithstanding. This is obviously of importance to fishery biologists who are concerned to explore the food chains of the valuable and commercially exploited tunas.

Several bramids are directly exploited as food by man. One such fishery off the western and northern coasts of Spain for *Brama brama* is locally important, the bramids being captured on long lines set some 18 to 25 miles offshore. The catch is marketed locally fresh or canned for use elsewhere in Spain. It is this species which from time to time makes an appearance in the waters of

Messenger RNA Inactivation

ONE of the diagnostic characteristics of messenger RNAs in bacteria is that they are labile and compared with ribosomal or transfer RNAs have very short half lives. For more than a decade, however, since this fact was established the molecular basis of the instability of messenger RNA has remained an enigma. To be sure various ribonucleases have been discovered and tentatively implicated in mRNA decay, and the four obvious possible mechanisms of messenger degradation—namely that these molecules are degraded from one or both ends or internally at any position or at specific sites—have had their supporters, but the issue is still unresolved.

One of the problems, as Blundell, Craig and Kendall point out in next Wednesday's *Nature New Biology* (July 12), is that different bacterial strains and different growth conditions have been used by groups measuring the kinetics of turnover of this or that messenger, and not surprisingly the resulting data are disparate.

By measuring the loss of activity of

enzymes specified by the *lac* and *trp* operons, and indirectly, therefore, measuring the turnover of the messengers of these operons, in the same *Escherichia coli* cells, Blundell *et al.* have at least avoided this unnecessary complication. And by exploiting various deletion mutants they have measured the half lives of shortened messenger molecules. Their experiments reveal two important characteristics of messenger inactivation; first, different species of messengers are inactivated at different rates and, second, within one polycistronic RNA the different cistronic messages are also inactivated at different rates.

From these and other findings the group concludes that the most satisfactory model of the inactivation of messenger RNAs ascribes to each cistronic region of a polycistronic messenger a certain limited number of sites vulnerable to nucleolytic attack. Blundell *et al.* hope that by using the many deletion mutants of the *lac* operon it may be possible to identify these target sites.

northern Europe, especially the North Sea where it is frequently found stranded on the shore in winter.

Brama is the most numerous of the bramid genera and seven species are recognized. In the Atlantic, Mead finds that *B. brama* is confined to the northern temperate Atlantic and Mediterranean; another species, *B. dussumieri*, replaces it to the south (and also occurs in the Indian and Pacific Oceans). A third species is found in the Caribbean Sea and the tropical western Atlantic. The two species *B. brama* and *B. dussumieri* seem to have mutually exclusive ranges based, Mead deduces, on the limits set by temperature, that of 23.8° C being critical.

Clearly much remains to be learned of bramid biology. The north Atlantic *Brama brama* is relatively well known, but its season and area of breeding are still unknown and its diet is unstudied; all of the other bramids are less well known than this. Mead's study, with its analysis of the literature (painstakingly detailed through to the mid-nineteen-sixties, but strangely only a few general works cited later) and of a large number of specimens, provides a very useful guide for further work.

DNA STRUCTURE

Geometrical Versatility

from our Molecular Biology Correspondent

THE crystallographer's hobgoblin, that the structure in his crystal or fibre is not exactly that of the same molecule in solution, has been of late manifesting itself rather strongly in regard to DNA. Until recently three structures were recognized in fibres, and these—the A, B and C forms—could be made to interconvert in response to changes in humidity and salt. The evidence is that in dilute solution DNA is in the B form, which is found in fibres at high humidity. Just how this relates to DNA in the cell, be it in the nucleohistone of mammalian chromosomes, in nucleoprotamine or in phage heads, is by no means clear, and there are some indications that the answer may not be altogether simple.

The optical activity of two-stranded DNA is highly sensitive to the helix geometry, and therefore serves as a delicate, if only in a rudimentary sense interpretable, marker for its response to changes in its environment. It has been abundantly demonstrated that in dispersed nucleohistone the circular dichroism differs from that of DNA in free solution, and resembles rather closely that of the C form. In complexes of DNA with some histone fractions, on the other hand, startlingly different curves are obtained, with inversion of Cotton effects. Disregarding the awkward possibility of contributions

from optical artefacts, which are difficult to rule out in work on particulate samples, it seems as though the phenomena in themselves may be highly specific.

A recent example concerns the complex of DNA with the lysine-rich histone, F1. Gottesfeld *et al.* (*Biochemistry*, **11**, 1422; 1972) find that the inversion of the DNA circular dichroism, which occurs in this system, is provoked more readily by the homologous F1 fraction than that from other animal sources, but that this species specificity is lost after purification of the histones by ion-exchange chromatography. Crudely interpreted, this means that the specificity of interaction is promoted by another kind of molecule. Again, the smaller circular dichroism disturbance induced in DNA by histone IV is modified by calcium ions (Wagner and Vandergrift, *ibid.*, 1431).

It is not, however, only in these complex systems that the optical properties of DNA undergo large changes. High salt concentrations and the addition of non-aqueous solvents can encompass such effects in dilute isotropic solutions, and it has long been known that increasing temperature, below the melting region, causes physical changes in DNA, which are manifested in various ways, including an increase in circular dichroism, and as Palecek and Fric (*Biochem. Biophys. Res. Commun.*, **47**,

GEOLOGY

Ulster Surveyed

THE series of handbooks on the "Regional Geology of Great Britain" has for some 30 years been a much valued source of reference for both amateur and professional geologists. The Geological Survey has now extended the series to Northern Ireland (HMSO, 50p) and the volume effectively shows the variety of geological features of all types in the region—from the Moinian to the Quaternary (with the exception of the Cambrian) together with many examples of glacial and recent phenomena. It is expected that a new map of Ulster on the scale of 1/250,000 will be published soon after this handbook.

1262; 1971) report, a progressive increase in polarographic reducibility. Gennis and Cantor (*J. Mol. Biol.*, **65**, 381; 1972) found that the circular dichroism change is largest in poly dAT, that it occurs in glucosylated DNA, and that it is independent of salt concentration. It is thus evidently not to be identified with a transition to the A form, but represents some kind of geometrical adjustment.

An interesting investigation of the

Transcription and Excision of Prophage Lambda

IN next Wednesday's *Nature New Biology* (July 12) Davies, Dove, Inokuchi, Lehman and Roehrdanz describe a set of ingenious experiments with lambda lysogens which clearly indicate that the rate of excision of lambda prophages from the chromosomes of their *Escherichia coli* hosts is regulated at least in part by local transcription of the prophage DNA. This finding, together with the previous observation, made by this and other groups, that the initiation of DNA replication is intimately dependent upon transcription of certain regions of the DNA to be replicated, indicates that the control of bacteriophage, and probably bacterial gene replication, gene excision and gene expression, are integrated processes considerably more subtle than most people have envisaged.

Davies *et al.* exploited to the full the sophisticated techniques of genetic manipulation which are now available to the bacterial geneticists, especially to those who work with phage lambda. They constructed a strain of *E. coli* carrying a lambda prophage which because of mutation cannot make the so-called N gene product which is required for the transcription of most of the lambda genome. They then super-

infected these lysogens with one or other of two helper phages and measured indirectly the excision of the defective lambda prophage genome.

Both helper phages provide all the enzymes required for the excision of the prophages and their subsequent replication as independent genomes, but only one of the two sorts of superinfecting helper phages used provides N gene product and therefore allows transcription of the lambda prophage before excision.

Results of this experiment, together with a variety of controls which rule out alternative explanations, lead to the conclusion that only when the prophage genome is being transcribed can it be excised to any significant extent. In short, local transcription directly changes the rate of excision, perhaps by altering the conformation or intracellular location of the prophage genome.

Any model to account for the control of excision must, therefore, subsume the mechanisms of the regulation of transcription, and although it may be convenient for experimentalists to regard DNA replication, transcription and translation as separate processes they are probably all closely interwoven.

effects of salt and temperature on DNA circular dichroism comes from Studdert, Patroni and Davis (*Biopolymers*, **11**, 761; 1972). Very high salt concentrations, especially of lithium salts, elicit large changes in ellipticity, and at low temperature an inversion of the longest wavelength Cotton effect. The effects of salt concentrations and temperature are related, and it is apparently possible to arrive at a given optical state by varying one or the other at will, an increase in salt concentration being equivalent to a decrease in temperature. In qualitative terms similar optical manifestations occur in the DNA in phage heads and in supercoiled covalently closed DNA. Poly dAT responds to high salt concentrations in much the same way as natural DNA (in spite of differences in conformational stability, for as Pilet and Brahms recently showed, it is unable to enter the A form in the solid state at low humidity).

The effect of a change in the tightness of winding of the DNA helix on the optical activity has been determined by Maestre and Wang in terms of supercoiled DNA, in which the number of turns of the secondary helix measures directly the change in the winding of the primary helix. On this basis it seems that increasing ionic strength and decreasing temperature cause progressive increase in the helix twist. The interesting feature of the observed optical change is its continuous, non-cooperative character. It is hard to escape the conclusion that DNA in solution can change its geometry within limits in a continuous manner.

Circular dichroism changes of this nature are also observed in the system of Lerman and his colleagues, which contains DNA, neutral polymers and salts. In these conditions the DNA undergoes a transition to a collapsed compact, rapidly sedimenting form, related, it is conjectured, to the condensed state in phage heads, for example. In a very similar system, involving polyethylene glycol as the neutral polymer, Evdokimov *et al.* (*FEBS Lett.*, **23**, 180; 1972) have independently found a progressive change in circular dichroism, culminating in inversion of the band, and the formation of particles of the order of 0.1μ in size. The compactness, like the magnitude of the circular dichroism, depends on the polymer concentration, and at high concentrations the particles, as seen in the electron microscope, are annular and entirely similar to those found in DNA-histone complexes. The large geometrical versatility which all of this work imputes to DNA is most probably simply an amplified response to very small changes in the geometry of the primary helix, which in all these systems can be presumed to be throughout, at least approximately, in the B conformation.

Abortive Transcription as Reovirus Matures

In next Wednesday's *Nature New Biology* (July 12) Bellamy, Nichols and Joklik report a series of experiments which clear up a question that has long puzzled virologists, namely the origin and significance of the many hundreds of very short (3–20 bases) single stranded RNA chains which are to be found in reovirus particles. These oligoribonucleotides, which are quite distinct from the ten double stranded RNA molecules that comprise the reovirus genome, fall into two classes by virtue of their base compositions. One class, more than 50 per cent of the total, is comprised of polyadenylic acid chains up to nineteen residues long. The second class contains molecules which are made up of the four RNA nucleotides but are rich in adenylic acid and uridylic acid.

By standard techniques Joklik's group isolated these two classes of molecules and determined their base sequences by methods devised by Sanger and his colleagues. Most of the poly A oligonucleotides proved to be $\text{ppp}(\text{Ap})_n\text{A}_{\text{OH}}$ where n ranges to 19. Most of the other molecules begin with the sequence pppGpCpUp and terminate in three or four adenylic and uridylic residues, the last having a 3' hydroxy group.

The fact that most of these oligori-

bonucleotides begin with a base carrying a 5' triphosphate group and end with a base with a free 3' hydroxy group suggests strongly, of course, that they are the products of transcription. Furthermore it is known that at least one class of reovirus mRNAs begins with the sequence pppGpCpCpUpAp —and other classes start with pppGpCp —. Bellamy *et al.* suggest therefore that the short oligonucleotides which begin pppGpCpUp —are the products of abortive transcription which occurs as the progeny virions are encapsidated. They suggest that capsid protein prevents the completion of transcripts and that the short abortive RNAs get trapped in the particles as the outer capsid matures.

The poly A molecules are perhaps harder to account for, but as Bellamy *et al.* suggest they may well be generated by RNA polymerase if it "slips" during the transcription of short runs of uridylic residues in the genomic RNA. A similar "slippage" mechanism has been invoked to explain the synthesis of poly A in *Escherichia coli* and elsewhere. In short, the group believe that the many short RNA chains in reovirus particles are accidental by-products of virus maturation and probably have no function.

Antibody Production and Skin Graft Rejection

SOME immunologists learn early, others later, that apparent failure to respond to an antigenic stimulus may relate only to the insensitivity of the method of measurement. This is particularly true of responses to complex stimuli such as skin grafts which probably elicit a response of many facets. Immunologists for convenience think of these responses as being of two kinds, those involving the production of humoral antibody and those which lead to the production of cytotoxic cells. It is popular to suppose that these last cell products effect the destruction of a rejected graft whereas the former antibodies, if at all effective, impede the hostile cells.

E. Goldberg *et al.* have investigated the phenomenon of female mice which fail to reject male skin grafts. It has been known for some years that the failure was not due to lack of the H-Y, male antigen, on the transferred skin. Goldberg *et al.* decided to seek cytotoxic anti-H-Y antibody in C3H female mice which had failed to reject a C3H male skin graft. They used either sperm or dissociated male epidermal cells as target cells. As they report in next Wednesday's *Nature New Biology* (July 12), they detected anti-H-Y antibodies in all animals. A few mice proved capable of rejecting a second male skin whilst the first was

retained. It was also seen that skin grafts tended to have a rejection crisis if the recipient mouse was splenectomized.

It thus seems that there are both humoral and cellular components to the anti-male response—the humoral response demonstrated by Goldberg *et al.*, the cellular response assumed because grafts can be rejected. One interpretation is that the two components of response are mutually antagonistic and that only when the balance between them is right will rejection occur.

A companion article by M. John *et al.* follows the same lines (E. A. Boyse is the common author). They show that A mice which have the θ -C3H antigen generally fail to reject skin grafts from the congenic A/ θ -AKR strain even though anti- θ -AKR antibody production is elicited. Grafting skin the other way round (A on to A/ θ -AKR) led to a rather different result—cytotoxic antibody production was transient and minimal and the grafts were usually rejected. John *et al.* point out that both the H-Y and θ antigens (and Sk, a skin antigen) are probably relatively weak histocompatibility antigens, but the pattern of response that they elicit may be similar to that against the so-called tumour specific antigens.

Parity and Optical Activity

L. D. BARRON

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Simple pictorial arguments can demonstrate that the natural and Faraday rotation experiments conserve parity and time-reversal invariance. The method can be used to predict possible new effects without recourse to mathematical theories.

OPTICAL activity, both natural and that induced by magnetic fields, is of considerable importance in physics, chemistry and biology. The results of optical activity experiments, such as optical rotatory dispersion and circular dichroism, can be correlated with features of molecular structure without enquiring too deeply into the basic theory of the phenomenon. There has, however, been some confusion over the role of parity, and it has been implied that processes involving optically active molecules need not conserve parity¹. The confusion presumably arises because interest usually centres only on the optically active molecules, which are non-centrosymmetric, so that parity is not an intrinsic molecular symmetry operation. In fact, statements about conservation of parity can be made only with reference to the complete system, which consists of molecules in the presence of a polarized light beam, together with the magnetic field in the case of the Faraday effect.

This article demonstrates, by means of simple pictorial arguments, that natural and Faraday optical rotation do indeed conserve parity, together with time-reversal invariance, and that important predictions can be made without recourse to the complicated models that are required to construct mathematical theories of the effects. Simple symmetry arguments have been applied before to the Faraday effect². This article extends these arguments to natural and electric field-induced optical activity, and demonstrates that the rotation of an elliptically polarized light beam by one of the components of a racemic mixture would not violate parity conservation and time-reversal invariance, and is therefore a possible process.

Spatial Symmetry and Optical Activity

Fresnel's celebrated theory of optical rotation³ followed from his discovery of circularly polarized light, which enabled him to analyse optical rotation in terms of different refractive indices for right and left circularly polarized light. This immediately provided an insight into the symmetry requirements for an optically active medium. In the words of Fresnel⁴:

There are certain refracting media, such as quartz in the direction of its axis, turpentine, essence of lemon, etc., which have the property of not transmitting with the same velocity circular vibrations from right to left and those from left to right. This may result from a peculiar constitution of the refracting medium, or of its atoms, which produces a difference between the directions right to left and left to right; such, for instance, would be a helicoidal arrangement of the molecules of the medium, which would present inverse properties according as these helices were dextrogyrate or laevogyrate.

A finite cylindrical helix is the archetype for all figures

exhibiting what Pasteur called dissymmetry to describe objects which cannot be superposed on their mirror images. Thus a helix and its mirror image cannot be superposed because reflexion reverses the screw-sense or chirality (Fig. 1). Figures which exist in two non-superposable mirror image forms are said to exhibit enantiomorphism. Dissymmetric figures are not necessarily asymmetric—devoid of all symmetry elements—because they may possess one or more pure rotation axes (the finite cylindrical helix has a two-fold rotation axis C_2 through the mid-point of the coil, perpendicular to the long axis). Dissymmetry excludes, however, a centre of inversion, reflexion planes, and rotation-reflexion axes.

Although the absence of a centre of inversion, reflexion planes, and rotation-reflexion axes in individual molecules is mandatory if an isotropic ensemble of the molecules is to show optical activity, it is not widely appreciated that some crystals and oriented molecules which lack a centre of inversion but possess reflexion planes or a rotation-reflexion axis (so that they are superposable on their mirror images) can show optical activity for certain directions of the light beam⁵. For example, an oriented water molecule, which has two reflexion planes (C_{2v}), appears as part of a helix to a light beam propagating in any direction not contained in either of the two reflexion planes (Fig. 2). For every direction of the light beam for which the molecule appears as part of a left handed helix, there is a direction for which the molecule appears as part of a corresponding right handed helix. The optical rotations in the two directions are equal and opposite, so an isotropic ensemble of water molecules does not show optical activity. Although optical activity in oriented water has never been observed, optical rotation has been observed in certain directions in two other non-enantiomorphous systems: crystals of silver gallium sulphide, which have two symmetry planes and a rotation-reflexion axis (D_{2d} , ref. 6), and nematic liquid crystals of planar para-azoxyanisole molecules (C_s) oriented on a glass plate in a magnetic field perpendicular to the direction of propagation of the light^{7,8}. Thus optical activity is not exclusively related to enantiomorphism. In the following discussion it is assumed for simplicity that natural optical activity is associated only with enantiomorphism (dissymmetry), because the conclusions apply equally to non-enantiomorphic natural optical activity.

Inversion Symmetry and Physical Laws

The discussion so far has been concerned with the intrinsic spatial symmetry, known as point group symmetry, of optically active molecules and crystals. An object is said to have spatial symmetry if, after subjecting it to a symmetry operation such as inversion, reflexion, or rotation with respect to a symmetry element within the object, it looks exactly the same as it did before. But even more remarkable than the spatial symmetry of the objects of the physical world is the fact that symmetries exist in the laws which determine the operation of the physical world. In particular, if a complete experiment is subjected to space-inversion or time-inversion, the resulting experiment should, in principle, be realizable^{9,10}. The symmetry operation of space-inversion, represented by the parity operator P , inverts the coordinates used to specify the system through the coordinate origin, which may be located arbitrarily. This is equivalent to a reflexion of the actual physical system in any plane containing the coordinate origin, followed by a

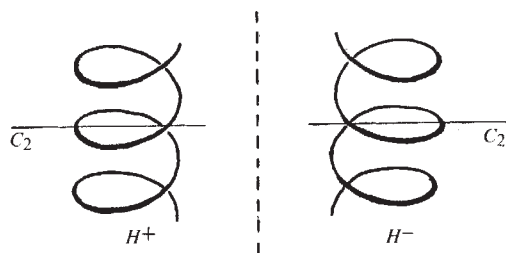


Fig. 1 A right handed helix H^+ and its left handed mirror image H^- .

rotation of 180° about a line perpendicular to the reflexion plane. Most physical laws, in particular those of electromagnetism (but not those responsible for β -decay) are unchanged by space-inversion. In other words, the equations representing the physical laws are unchanged if the coordinates (x, y, z) are replaced everywhere by $(-x, -y, -z)$, and the physical processes described by those laws are said to conserve parity. The symmetry operation of time-reversal, represented by the operator T , reverses the motions of all the physical entities in the system. If replacement of the time coordinate (t) by $(-t)$ everywhere in equations representing physical laws leaves those equations unchanged, the physical processes described by those laws are said to be time-reversal invariant.

Physical quantities are represented by scalars, vectors, and tensors, which can be classified according to their behaviour under the operations P and T (ref. 11). Vectors whose signs are changed by P are called polar, or true, vectors—for example, the position vector $\mathbf{r}$. Vectors whose signs are not changed by P are called axial, or pseudo, vectors—for example, the angular momentum vector is defined as $\mathbf{L} = \mathbf{r} \times \mathbf{p}$, the vector product of the position $\mathbf{r}$ and momentum $\mathbf{p}$, and as the polar vectors $\mathbf{r}$ and $\mathbf{p}$ change sign under space-inversion, the axial vector $\mathbf{L}$ does not. In other words, $\mathbf{L}$ is defined relative to the sense of rotation by a “right hand” rule, and P does not change the sense of rotation. Vectors whose signs are not changed by T are called time-symmetric; for example, the position vector $\mathbf{r}$. Vectors whose signs are changed by T are called time-antisymmetric; for example, velocity $\mathbf{v}$ and angular momentum $\mathbf{L}$.

We are particularly interested in the behaviour of the electric and magnetic field vectors $\mathbf{E}$ and $\mathbf{H}$ under the operations P and T . This is best determined by examining the symmetry of the physical systems which generate $\mathbf{E}$ and $\mathbf{H}$ (ref. 11). For example, an electric field can be generated by a pair of parallel plates carrying equal and opposite charge densities. Under P , the two plates exchange positions, although retaining their respective charges, so $\mathbf{E}$ changes sign. As the charges are stationary, T does not affect the system. Thus $\mathbf{E}$ is a polar, time-symmetric, vector. A magnetic field can be generated by a cylindrical current sheet. The sense of rotation of the electrons around the current sheet is reversed by T but not by P . Thus $\mathbf{H}$ is an axial, time-antisymmetric, vector.

We can now demonstrate that the laws of electromagnetism are invariant under the operations P and T . The laws are summarized by Maxwell's equations and the Lorentz force equation:

$$\begin{aligned}\nabla \cdot \mathbf{D} &= \rho, \quad \nabla \cdot \mathbf{B} = 0 \\ \nabla \times \mathbf{E} &= -\frac{\partial \mathbf{B}}{\partial t}, \quad \nabla \times \mathbf{H} = \mathbf{J} + \frac{\partial \mathbf{D}}{\partial t} \\ \mathbf{F} &= \rho \mathbf{E} + \mathbf{J} \times \mathbf{B}\end{aligned}$$

where ρ (a time-symmetric scalar) and $\mathbf{J}$ (a time-antisymmetric polar vector) are the charge and current densities, and $\mathbf{D}$ and $\mathbf{B}$ (which transform the same as $\mathbf{E}$ and $\mathbf{H}$) are the electric displacement and magnetic induction. Thus the third equation, for example, which is Faraday's and Lenz's law of electromagnetic induction, is invariant under P and T :

$$\nabla \times \mathbf{E} = -\frac{\partial \mathbf{B}}{\partial t} \left\{ \begin{array}{l} P \rightarrow (-\nabla) \times (-\mathbf{E}) = -\frac{\partial (+\mathbf{B})}{\partial (+t)} \\ T \rightarrow (+\nabla) \times (+\mathbf{E}) = -\frac{\partial (-\mathbf{B})}{\partial (-t)} \end{array} \right.$$

The remaining equations are easily shown to be similarly invariant. Consequently, any physical process involving only electromagnetic interactions, for example the interaction of light with molecules, must conserve parity and be invariant under time-reversal.

Inversion Symmetry and Optical Rotation

The natural optical rotation experiment consists of a dissymmetric refractive medium, such as a quartz crystal or a fluid or solution composed of inherently dissymmetric molecules, in which the electric field vectors of a linearly polarized light beam are established in a helical pattern; the orientation of each electric field vector is a function only of its position in the medium, although its amplitude is a function of time. A convenient representation of this helical pattern is a twisted ribbon extending through the medium, with the electric field vectors vibrating in the plane of the ribbon (Fig. 3a). The helical pattern of electric field vectors is a physical object with well defined symmetry properties. Because only electromagnetic interactions are involved, the physical process giving rise to optical rotation must conserve parity and be invariant under time-reversal. In other words, if P and T are applied to the entire experiment, the result must also be a possible experiment. Thus under P , the chirality of the helical pattern of electric field vectors in the medium is reversed (corresponding to a reversal of the sense of optical rotation), together with the direction of propagation of the light; at the same time the medium is converted into its non-superposable mirror image (Fig. 3a, b). This result is a possible experiment, because replacing the dissymmetric medium with its enantiomorph results in an opposite sense of optical rotation, and reversing the direction of the light beam does not affect the sense of the optical rotation (defined by an observer receiving the light). Thus natural optical rotation conserves parity. Under T , the direction of propagation of the light is reversed, but the chirality of the helical pattern of electric field vectors in the medium is preserved (Fig. 3a). As T does not affect the dissymmetric medium, if it is non-magnetic (Fig. 3b), the time-reversed experiment is physically realizable and corresponds simply to reversing the direction of propagation of the light. This does not change the sense of optical rotation, so natural optical rotation is invariant under time-reversal.

The Faraday rotation experiment consists of a non-dissymmetric medium in a static, externally applied magnetic field parallel to the direction of propagation of a linearly polarized light beam whose electric field vectors are established in a helical pattern in the medium. Again P reverses the direction of propagation of the light beam and the sense of optical rotation, but the medium and the magnetic field are

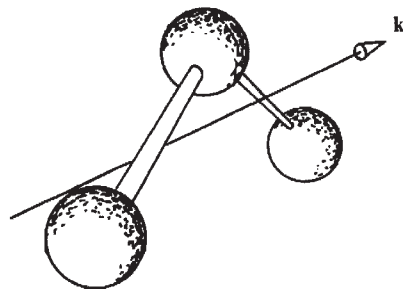


Fig. 2 An oriented water molecule which appears as part of a left handed helix to a light beam propagating parallel or antiparallel to $\mathbf{k}$.

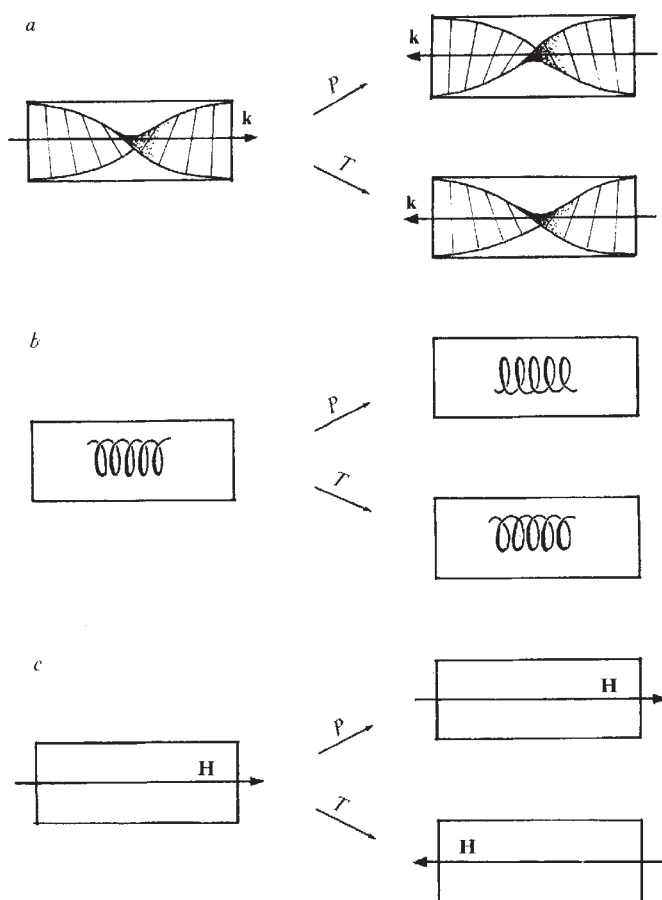


Fig. 3 The effect of P and T . a , On the helical pattern of electric field vectors of a light beam in a rotatory medium; b , on a dissymmetric medium; c , on a non-dissymmetric medium in a magnetic field.

unchanged (Fig. 3a and c). This corresponds with reality, because it is found experimentally that reversing the direction of propagation of the light relative to the magnetic field direction reverses the sense of the Faraday rotation. Thus Faraday rotation conserves parity. Under T , the direction of propagation of the light is reversed, but its sense of optical rotation is preserved. As T reverses the direction of the magnetic field but does not affect the medium, if non-magnetic (Fig. 3c), the time-reversed experiment is physically realizable, for reversing the directions of both the magnetic field and the light preserves the sense of the Faraday rotation. Thus Faraday rotation is invariant under time-reversal. One can also see that Faraday rotation must depend on odd powers of H , because these change sign under T whereas even powers of H do not.

Pictorial arguments can also be used to demonstrate that there is no simple electrical analogue of the Faraday effect¹²; in other words, that optical rotation cannot be induced in linearly polarized light in an isotropic non-dissymmetric medium by a static electric field in the direction of propagation of the light beam. Thus P does not affect the medium, although the direction of the electric field and the direction of propagation and sense of optical rotation of the light beam are reversed: as all directions in the unperturbed medium are equivalent, any optical rotation induced by odd powers of E would violate parity. The extension of these pictorial arguments to more exotic media is cumbersome, and I shall refer to group theoretical discussions to show that electric field-induced optical rotation is possible in certain crystals¹³ (electric field-induced optical rotation has recently been observed in ferromagnetic crystals of chromic oxide¹⁴).

Although rotation of the plane of polarization of linearly polarized light in a medium which is not naturally optically

active, in the absence of magnetic fields, would violate parity, rotation of the major axis of the polarization ellipse of elliptically polarized light in the same medium would not. Elliptically polarized light is a coherent superposition of linearly and circularly polarized components. The tip of the electric field vector in a fixed plane perpendicular to the direction of propagation of circularly polarized light traces out a circle with time. Thus P reverses the handedness of circularly polarized light because the rotation sense of the electric field vector is maintained, but the direction of propagation is reversed. The handedness of circularly polarized light is maintained under T , because both the rotation sense of the electric field vector and the direction of propagation are reversed. Thus, under P , the medium is not affected, but the direction of propagation, the sense of optical rotation, and the handedness of the ellipticity of an elliptically polarized light beam are reversed; that is, reversing the handedness of the ellipticity reverses the sense of the optical rotation. Under T , the medium is not affected, and the sense of the optical rotation and the handedness of the ellipticity are maintained, but the direction of propagation of the light is reversed. These conclusions agree with observations of the effect known as the auto-rotation of the polarization ellipse, in which the major axis of the polarization ellipse of an intense elliptically polarized laser beam rotates on passing through an isotropic non-optically active fluid, on account of a partial orientation of the molecules by the intense optical electric field: reversing the handedness of the ellipticity reverses the sense of the optical rotation¹⁵. An interesting speculation is that a mechanism could exist for producing optical rotation of elliptically polarized light in a racemic mixture, which would be a function of the optical activity of one of the enantiomorphs; such an effect would not violate parity or time-reversal invariance.

The pictorial demonstration that optical rotation conserves parity can be augmented mathematically by considering equations for the angles of rotation in terms of molecular quantities. For example, Rosenfeld¹⁶ derived the following equation for the angle of natural optical rotation per unit path length:

$$\theta = (2\hbar\omega^2 \mu_0/3) I_m \sum_c \left\{ \frac{\mu_{ge} \cdot \mathbf{m}_{eg}}{(E_e - E_g)^2 - \hbar^2 \omega^2} \right\}$$

where μ_{ge} and $\mathbf{m}_{eg}$ are the matrix elements of the electric and magnetic dipole moment operators between the ground and excited states of the molecule. The left hand side is the pitch of the helical pattern of electric field vectors established by the light beam in the rotatory medium; it is therefore a scalar which changes sign under P (a pseudoscalar). The right hand side involves the scalar product of the polar vector μ_{ge} and the axial vector $\mathbf{m}_{eg}$ and is also a pseudoscalar. Thus Rosenfeld's equation conserves parity.

I thank Professor A. D. Buckingham and members of his research group for discussion.

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Can the Lumpy Distribution of Galaxies be Detected by X-ray Observations?

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Published data from X-ray telescopes are not adequate to reveal the fluctuations in the surface brightness of the background radiation caused by clustering or superclustering of the sources giving rise to the background.

A POTENTIALLY useful method of discriminating between theories of the origin of the diffuse cosmic X-ray background radiation^{1,2} is to examine the data from X-ray telescopes for fluctuations in the surface brightness of the sky on small angular scales. These fluctuations are to be expected if the sources of the X-rays are clustered as galaxies are clustered (or are superclustered as clusters of galaxies themselves may be superclustered). Thus, if the data show evidence of this clustering, most of the background radiation must be coming from objects at small redshifts, and theories which are based on processes at large redshifts can be discarded.

In both analyses published so far^{3,4} the conclusion is reached that the brightness of the sky is too smooth for the radiation to originate in objects clustered or superclustered like galaxies. Wolfe and Burbidge³ inferred from this result either that the background originates at large redshifts, or that it originates in objects at small redshifts which are smoothly distributed in space and are not clustered.

Unfortunately, however, both analyses^{3,4} are erroneous, and the published X-ray data are inadequate to detect fluctuations in brightness with the amplitude and scale size predicted from calculations based on a model universe in which the X-ray sources are completely clustered and superclustered. The errors occurred both in the calculation of the predicted fluctuations and in the presentation of the observational material, and this article contains a re-analysis of the problem.

Clustering

To calculate the strength of the fluctuations due to clusters of galaxies it is necessary to know their size and mean density in space and to have some idea of how the mean density of the matter within a cluster falls with distance from the centre. These data may be inferred from an analysis of the distribution of the images of galaxies on the plates of the Lick survey by Neymann *et al.*⁵, who found that the distribution of the images is consistent with the assumptions that all galaxies are cluster members; cluster centres are randomly distributed in space; and the distribution of galaxies within a cluster follows a Gaussian law:

$$P(\eta) dV = \text{const.} \exp \left\{ \frac{-\eta^2}{2\sigma^2} \right\} dV \quad (1)$$

where $P(\eta)dV$ is the probability of finding a galaxy in the small element of volume dV at distance η from the cluster centre, and σ is a constant (the "size" of a cluster).

The analysis gave a best estimate of 2.6 Mpc for σ (this and other values quoted here have been referred to the value $H = 75 \text{ km s}^{-1} \text{ Mpc}^{-1}$ for Hubble's constant at the present epoch), but λ , the mean density of clusters in space, was not

directly specified. n , the mean density of galaxies in space, was determined to be about 0.1 Mpc^{-3} , so the relationship $n = \lambda N$ (where N is the mean number of galaxies per cluster) will be used here to estimate λ . N itself is not well determined: Neymann *et al.* found only that it cannot be more than a few hundred, and I shall assume it to lie in the range $30 \leq N \leq 300$. Thus for $N = 100$ galaxies per cluster, $\lambda = 0.001 \text{ cluster centres Mpc}^{-3}$. The reciprocal of λ is V , the mean volume per cluster:

$$V = 1/\lambda \text{ Mpc}^3 \quad (2)$$

and V may be written

$$V = L^3 \quad (3)$$

which defines L , the typical separation of cluster centres.

Next I assume that the X-rays originating in each cluster come from a large volume of space, the emissivity of the cluster depending on the distance from the cluster centre in the same fashion as the galaxy probability function. Thus for the Gaussian distribution in equation (1)

$$\epsilon(\eta) dV = \epsilon_{\text{max}} \exp \left\{ \frac{-\eta^2}{2\sigma^2} \right\} dV \quad (4)$$

where ϵ is the emissivity (that is, the X-ray power radiated by unit volume of space) in the X-ray band in which observations are made. This assumption needs little justification if the radiation comes from the intergalactic medium⁶, but if the radiation comes from small, discrete sources within each cluster, as would be the case if the sources are galaxies, then the assumption is reasonable only if (1) the resolution of X-ray telescopes is insufficient to separate the individual sources within each cluster; and (2) there are many discrete sources in each cluster at any given time. These two conditions ensure that the cluster of discrete sources is indistinguishable from a fuzzy region with gradually varying emissivity (equation (4)). Condition (1) is met by the detectors from which data have been published, but condition (2) is not obviously satisfied. I shall assume for the present that, if the X-ray background comes from discrete sources at small redshifts, there is a large number of sources per cluster, and the consequence of the opposite assumption will be discussed later.

A Naive Calculation

To compare prediction with observation it is necessary to calculate the strength of the surface brightness fluctuations from the assumed distribution of emissivity. Wolfe and Burbidge³ did this by calculating the (2-dimensional) autocorrelation function (ACF) of the surface brightness, I , from the (3-dimensional) ACF of the emissivity, ϵ . This procedure is rather indirect, and involves some complicated mathematics, so before discussing their result a naive calculation is described which gives much the same answer. At the expense of a few simplifying assumptions a physical insight is given into the fluctuations of brightness on the variables describing the distribution of the emissivity.

First, it is assumed that, viewed from the Earth with a high-resolution X-ray telescope, each cluster would appear to be a disk, radius d Mpc, with uniform surface brightness. This surface brightness, B , is assumed to be the same for all

clusters. Second, the universe is assumed to be Euclidean, isotropic and static. To avoid Olber's paradox, which would result if the universe were also assumed to be infinite in extent, the distribution of clusters is truncated at a distance from the Earth equal to the Hubble radius R_H :

$$R_H = c/H \quad (5)$$

where c is the velocity of light. In other words, the universe is assumed to be a sphere, inside which the clusters are distributed at random, and outside which there are no clusters at all (Fig. 1).

The surface brightness observed in a given direction is equal to the product of the number of clusters intercepted by the line of sight and the surface brightness of a cluster. The mean brightness of the sky, $\bar{I}$, is therefore proportional to $\bar{\mathcal{N}}$, the mean number of clusters intercepted. $\bar{\mathcal{N}}$ is equal to the expected number of clusters whose centres lie within a distance d of the line of sight:

$$\bar{\mathcal{N}} = \pi d^2 R_H \lambda \quad (6)$$

The cluster centres are assumed to be distributed at random, so the expected r.m.s. value of the fluctuation in the number of clusters intercepted is $\bar{\mathcal{N}}^{1/2}$. Therefore the ratio of the r.m.s. fluctuation to the mean brightness is

$$\frac{\delta I}{\bar{I}} = \frac{B \bar{\mathcal{N}}^{1/2}}{B \bar{\mathcal{N}}} = \bar{\mathcal{N}}^{-1/2} = (\pi d^2 R_H \lambda)^{-1/2} \quad (7)$$

Substituting the values

$$\left. \begin{aligned} d &= \sigma = 2.6 \text{ Mpc} \\ R_H &= 4,000 \text{ Mpc} \\ \text{and } \lambda &= n/N = 0.1/N \text{ Mpc}^{-3} \end{aligned} \right\} \quad (8)$$

gives $\bar{\mathcal{N}} = 8,500/N$ and $\delta I/\bar{I} = 0.011 N^{1/2}$. Thus for the assumed value $N = 100$, $\bar{\mathcal{N}} = 85$ and $\delta I/\bar{I} = 0.11$.

This estimate of the strength of the fluctuations applies when comparing the brightness in two directions well apart in the sky. The brightnesses in two directions close together will be similar because the lines of sight in these directions will intercept the same clusters; therefore an angular scale is associated with the fluctuations. If two lines of sight are separated by an angle much greater than a few times the apparent angular diameter of a cluster at distance R_H , they will have few clusters in common, and the surface brightnesses will be uncorrelated. This angular scale may be written

$$\theta_0 = \alpha \frac{2d}{R_H} \quad (9)$$

where α is a numerical constant of order unity.

A Previous Calculation

The ACF calculation³ is based on more realistic assumptions regarding the cosmological effects and the distribution of emissivity within each cluster. In particular it is assumed that the Friedmann cosmology holds, the sizes of the clusters and of the superclusters evolve as do the sizes of the linearized perturbations of Friedmann models and the ACF of the X-ray emissivity of space is a Gaussian function:

$$\begin{aligned} f_3(\Delta \mathbf{r}) \equiv f_3(\Delta \mathbf{r}) &\equiv \frac{\overline{\delta \varepsilon(\mathbf{r}) \delta \varepsilon(\mathbf{r} + \Delta \mathbf{r})}}{\overline{\delta \varepsilon^2(\mathbf{r})}} \\ &= \exp \left\{ \frac{-k^2}{2} (\Delta \mathbf{r})^2 \mathbf{r} \right\} \end{aligned} \quad (10)$$

where $\delta \varepsilon$ is the fluctuating part of the emissivity (that is $\varepsilon(\mathbf{r}) = \bar{\varepsilon} + \delta \varepsilon(\mathbf{r})$, $\mathbf{r}$ is the position vector of a point in the usual comoving coordinates, $\Delta \mathbf{r} = |\Delta \mathbf{r}|$, k is the "correlation wave-number" which determines the scale size of the fluctuations

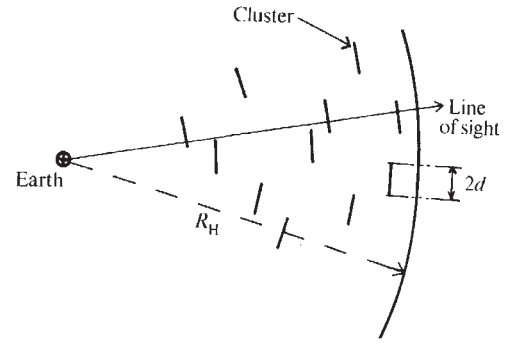


Fig. 1 Section through a sector of the simplified model universe.

in comoving coordinates, and the subscript on f denotes the dimensionality of that function.

The effects of redshift on the photon energies were allowed for by use of equations which are correct for a spectrum

$$\varepsilon(E) = \text{const. } E^{-1} \text{ erg eV}^{-1} \text{ s}^{-1} \text{ cm}^{-3} \quad (11)$$

(where E is the photon energy) which is not a bad approximation to the observed background spectrum^{1,2}.

The choice of a Gaussian for the ACF f_3 corresponds exactly to the assumption that the emissivity has a Gaussian distribution within each cluster: the ACF of the function defined in equation (4) is

$$f_3(\Delta \eta) = \exp \left\{ \frac{-(\Delta \eta)^2}{4\sigma^2} \right\}$$

so by comparison with equation (10)

$$k = R_H / \sigma \sqrt{2} \quad (12)$$

For $\sigma = 2.6$ Mpc and $R_H = 4,000$ Mpc, $k = 1,100$.

Wolfe and Burbidge showed that the ratio of the r.m.s. brightness to the mean expected under these conditions is approximately

$$\frac{\delta I}{\bar{I}} = 1.3 \left(\frac{\delta \varepsilon}{\bar{\varepsilon}} \right)_0 k^{-1/2} \quad (13)$$

where $\left(\frac{\delta \varepsilon}{\bar{\varepsilon}} \right)_0$ is the ratio of the r.m.s. emissivity to the mean at the present epoch. To compare this result with that from the naive calculation (equation (7)) it remains only to evaluate $\left(\frac{\delta \varepsilon}{\bar{\varepsilon}} \right)_0$ in terms of the variables σ and λ which define the distribution of ε . This evaluation is not simple to perform exactly because of the random distribution of the cluster centres, but the typical separation of cluster centres is several times greater than the cluster radius ($L/\sigma \sim 4$ for $N = 100$) so little error is introduced by assuming that each cluster is at the centre of an otherwise empty sphere of volume V (equation (2)), and

thus of radius ρ where $\frac{4\pi}{3} \rho^3 = V$. Therefore

$$\left(\frac{\delta \varepsilon}{\bar{\varepsilon}} \right)_0 = (\bar{\varepsilon}^2 - \bar{\varepsilon}^2)^{1/2} / \bar{\varepsilon} \quad (14)$$

where

$$\bar{\varepsilon} = \frac{3}{\rho^3} \int_0^\rho \eta^2 \varepsilon(\eta) d\eta$$

and similarly $\bar{\varepsilon}^2$. For $\varepsilon(\eta)$ defined by equation (4), $\left(\frac{\delta \varepsilon}{\bar{\varepsilon}} \right)_0$ is

a function only of the ratio ρ/σ ; this function is plotted in Fig. 2 for a range of values of the argument. For $\rho/\sigma \geq 2$ the function is closely equal to its asymptotic forms

$$\left(\frac{\delta \varepsilon}{\bar{\varepsilon}} \right)_0 = \frac{1}{6^{1/2} \pi^{1/4}} \left(\frac{\rho}{\sigma} \right)^{3/2} \quad (15)$$

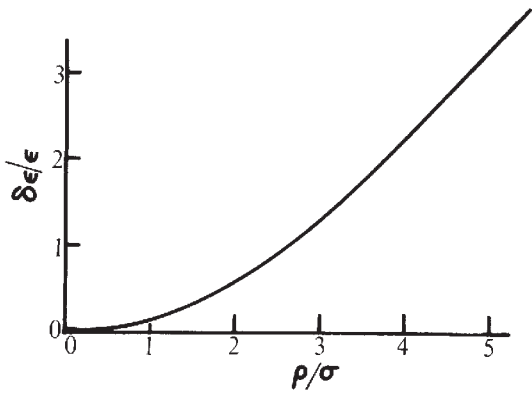


Fig. 2 R.m.s. fluctuations of the emissivity calculated for the case of a Gaussian dependence of the emissivity on distance from the cluster centre.

so in this case

$$\left(\frac{\delta\epsilon}{\epsilon}\right)_0 = 0.150 (\lambda\sigma^3)^{-1/2} \quad (16)$$

and finally equation (13) becomes

$$\frac{\delta I}{I} = \frac{0.23}{(\lambda R_H \sigma^2)^{1/2}} = 0.41 \times \left(\frac{\delta I}{I} \text{ calculated from equation (7)} \right) \quad (17)$$

Thus equation (7) from the naive model calculation with $d=\sigma$ differs from (17) only in the value of the multiplying constant. For $N=100$ galaxies per cluster, equation (17) gives

$$\frac{\delta I}{I} = 0.04$$

(The large value $\frac{\delta I}{I} \sim 1$ calculated by Wolfe and Burbidge for the case $N=100$ appears to be the result of erroneously replacing $\left(\frac{\delta\epsilon}{\epsilon}\right)_0$ by $\epsilon_{\max}/\bar{\epsilon}$ in equation (13). In fact they nowhere mention what estimate of $\left(\frac{\delta\epsilon}{\epsilon}\right)_0$ was used, but the

explanation proposed here accounts not only for the value calculated for $\delta I/I$, but also for the linear dependence of $\delta I/I$ on N evident in their Fig. 2; the correct dependence is seen from equations (7) and (17) in this article to be $\delta I/I \propto N^{1/2}$.)

The angular scale of the fluctuations expected from this ACF calculation may be represented by the angle $\theta_{1/2}$ at which $f_2(\theta)$, the ACF of the surface brightness, falls to the value 0.5. Plotting the curve of f_2 defined by equation (9) of ref. 3 gives

$$\theta_{1/2} \sim 9.7/k = 6.9 \left(\frac{2\sigma}{R_H} \right) \quad (18)$$

So once again the naive model predicts the same answer (equation (9)) to within a constant of multiplication.

Discussion

In Fig. 3 the expected values of $\delta I/I$ from equation (17) are compared with the upper limit from the observations of Schwartz *et al.*⁴. Both this upper limit and the expected angular scale Θ of the fluctuations are taken directly from ref. 3. Note that the beam convolution theorem which forms the basis of this comparison requires the use of the scale size Θ defined by equation (10) of ref. 3, which is related to the two-dimensional equivalent width of f_2 , and not the use of the scale size $\theta_{1/2}$, to which Θ is unrelated. The strength of the fluctuations is shown for the cases $N=30$, 100 and 300 galaxies per cluster. In each case the expected fluctuations are well below the observational limit (contrast Fig. 2 of ref. 3), so the observations are insufficiently sensitive to test the hypothesis that clustered X-ray sources give rise to the background radiation. Although there are uncertainties in the values of the parameters ρ and σ (refs.

5 and 8), and in the correct form of $\epsilon(\eta)$ (ref. 8), they are sufficiently small to have no effect on this conclusion.

Rocket Observations

Schwartz *et al.*⁴ recorded the photon intensity as the $2^\circ \times 2^\circ$ beam of a rocket-borne detector slowly scanned about 23° of the arc of a great circle in the sky. They found that the ACF of the record bore no resemblance to any of the ACFs calculated by Wolfe and Burbidge—indeed it looked like the ACFs of a smooth background intensity on which random noise had been superimposed by the process of measurement—so Schwartz *et al.* concluded that the brightness of the X-ray sky is too uniform for the background to have its origin in superclustered objects, and probably too uniform to have its origin in clustered objects.

This approach, of comparing observed and predicted ACFs, is correct when applied to data which are not appreciably contaminated by noise, but the presence of noise in the data systematically biases the curve of the observed ACF away from the curve of the ACF which would be derived from noiseless data. Schwartz *et al.* took no account of this effect in their analysis, and as a result their conclusion is incorrect.

Fig. 4 shows the effects of noise on the observed ACF. Fig. 4a represents the X-ray brightness of the sky as a function of angle θ along the arc of a great circle, and this signal shows fluctuations of the kind expected from clustering and superclustering. The signal is shown sampled at discrete intervals, defining M "bins" or "data points". The ACF is defined as

$$f_2(\theta) = g(\theta)/g(0) \quad (19)$$

where

$$g(\theta) = \frac{1}{M-k} \sum_{i=1}^{M-k} (\bar{I}_i - \bar{I})(I_{i+k} - \bar{I}) \quad (20)$$

and

$$\bar{I} = \frac{1}{M} \sum_{i=1}^M I_i$$

f_2 is a decreasing function of θ falling from unity at zero lag angle to value close to zero for large lag angles.

The points in Fig. 4b represent samples of a Gaussian noise signal, whose mean value is zero. $f_2(\theta)$, the ACF of this noise signal, itself looks like Gaussian noise with zero mean for all non-zero lags. But at $\theta=0$ the value of f_2 is unity. For large M this value is much greater than the r.m.s. value of $f_2(\theta>0)$ (see equation (22)), so the plot of the ACF shows a "spike" at $\theta=0$ which exists because at $\theta=0$ the noise adds coherently, whereas at $\theta>0$ it adds incoherently. When $\theta=0$ the function g is the sum of M terms, each of which is a square and is therefore positive:

$$g(0) = \frac{1}{M} \sum_{i=1}^M (I_i - \bar{I})^2 \quad (21)$$

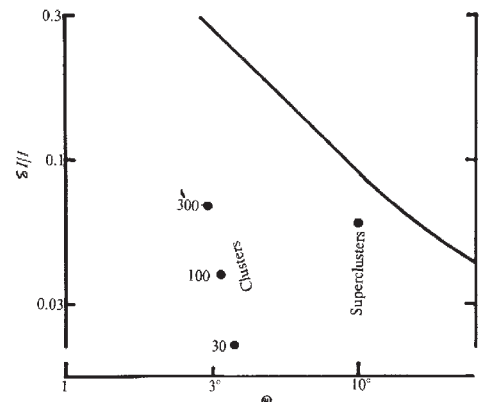


Fig. 3 Comparison of the predicted fluctuations on various angular scales with the upper limit from observation (solid line). The three points representing cluster models are for 30, 100 and 300 galaxies per cluster respectively.

For $k > 0$, however, $g(\theta)$ is the sum of $M - k$ terms which may have either sign, because I_i and I_{i+k} are uncorrelated in the noise waveform. Thus the expected value of g (and thus of f_2) for $\theta > 0$ is zero. Each term in the sum (equation (20)) is a random variable, so the sum will seldom be exactly zero. It is simple to show that

$$\frac{\text{r.m.s. value of } g(\theta) \text{ for } \theta > 0}{g(0)} \simeq \frac{1}{M^{1/2}} \quad (22)$$

for small lag angles.

In Fig. 4c the signal and the noise have been added together. f_2 , the ACF of this combined brightness distribution, still has a spike at $\theta = 0$ because there the noise adds coherently as before. The value of $f_2(0)$ is unity of course, so f_2 for small non-zero lag angles is considerably less than unity on account of the spike. But the values of f_2 for small lag angles are only a little less than unity, so the addition of noise to the signal has had the effect of shifting the ACF towards the line $f_2 = 0$. In other words, the addition of noise to I not only adds noise to the ACF of I , but also systematically biases the noisy ACF away from the ACF of the noiseless data. The greater the r.m.s. value of the noise added to the signal, the greater is this systematic bias, and the more closely does the ACF approach f_2 , the ACF of pure noise.

Schwartz *et al.* divided the photon counts into $M = 220$ bins, which gave a mean count of 5.5 photons per bin. The photons arrived at random times, so the actual number in any given bin is distributed like a Poisson variable with mean value 5.5, and with standard deviation $\sqrt{5.5} = 2.34$. Therefore the variation of the number of counts from bin to bin would have this r.m.s. value even in the absence of fluctuations of the surface brightness of the X-ray sky. The r.m.s. brightness fluctuations due to clustering have been shown to be only of the order of 4%, so the signal to noise ratio from bin to bin is exceedingly poor, only $0.04/(2.34/5.5) = 0.095$. In this limit of very small signal to noise ratio the ACF f_2 is a noisy but unbiased estimate of the function ϕ_2 , defined as

$$\phi_2(\theta) = \frac{\left(\frac{\delta I}{I}\right)_{\text{signal}}^2}{\left(\frac{\delta I}{I}\right)_{\text{noise}}^2} \cdot f_2(\theta) \quad (23)$$

where $\left(\frac{\delta I}{I}\right)_{\text{signal}}$ is the r.m.s. value of the fractional fluctuations in the background surface brightness; $\left(\frac{\delta I}{I}\right)_{\text{noise}}$ is the r.m.s. value of the fractional noise fluctuations from bin to bin; and f_2 is the ACF which would be calculated from noiseless data. $\left(\frac{\delta I}{I}\right)_{\text{noise}}$ is equal to $m^{-1/2}$, where m is the average number of photons per bin, so

$$\phi_2(\theta) = m \left(\frac{\delta I}{I}\right)_{\text{signal}}^2 \cdot f_2 \quad (24)$$

This function ϕ_2 is plotted in Fig. 5 with the ACF of the data from ref. 4. f_2 in equation (25) was taken from ref. 3 for the case $N = 100$. It is clear that the data are inadequate to distinguish whether the observed ACF more closely resembles the function ϕ_2 expected from the clustering, or the function $f_2 \equiv 0$ expected from a smooth background.

Possible Superclustering

The results of Karachentsev⁷ are representative of those analyses which suggest that clusters of galaxies are themselves bunched together in space to form superclusters. He found that the distribution of cluster centres in space is consistent with the hypothesis that all clusters are members of super-

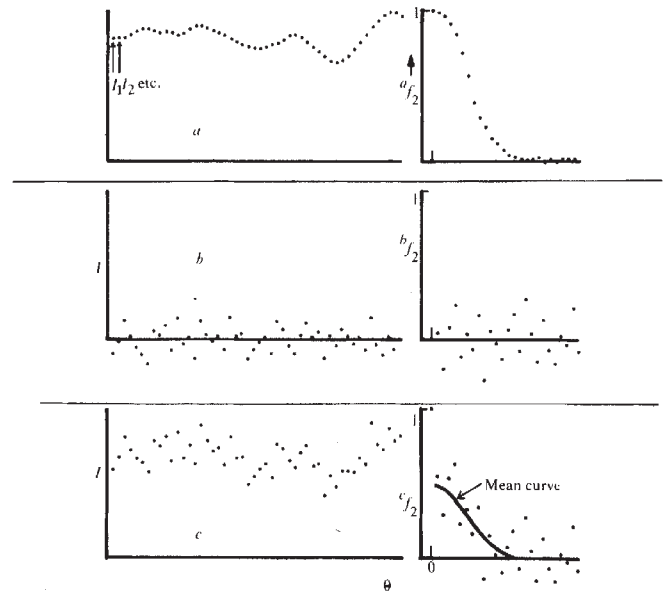


Fig. 4 Illustrating the effect of noise on ACFs. *a*, Signal; *b*, noise; *c*, signal and noise. In each of the three pairs of graphs, the right-hand graph is the ACF of the function sketched in the left-hand graph. Note that f_2 , the ACF of the noisy signal, is not a noisy estimate of f_2 , the ACF of the noiseless signal; it is a noisy estimate of a different function, shown as the mean curve (solid line) in *c*.

clusters, with about ten clusters in each supercluster. The average diameter of these superclusters is 42 Mpc, and the typical separation between supercluster centres, corresponding to L in equation (3), is 64 Mpc.

I have repeated the analysis using these values (that is, $\sigma = 21$ Mpc and $L = 64$ Mpc) to estimate the strength of the X-ray brightness fluctuations due to possible superclustering. There is no indication what the actual form of the density distribution within these superclusters is, so a Gaussian distribution is again assumed, for want of a better hypothesis.

Now $L/\sigma = 3.0$ so $\rho/\sigma = 1.9$. Fig. 2 gives $\left(\frac{\delta \epsilon}{\epsilon}\right)_0 = 0.50$ for this value of ρ/σ . From equation (12) $k = 135$ so finally equation (13) gives $\delta I/I \simeq 0.06$. This value is plotted in Fig. 3, again taking Θ from ref. 3. Also, a second curve ϕ_2 is plotted in Fig. 5, calculated from equation (24) with f_2 now one of the curves for superclustering from ref. 3. It is apparent from both figures that the data are insufficient to rule out the possibility that the X-ray background arises in superclustered objects.

The estimate of $\delta I/I$ for superclusters is much less accurate than that for clusters because the parameters describing the superclusters are poorly known. The only way in which this calculation might prove useful is if improved X-ray observations actually detect fluctuations with roughly the right amplitude and scale size ($\simeq 4^\circ$ from equation (18)) for superclustering. This would be strong evidence that superclusters indeed exist, and also that most of the X-ray background comes from low-redshift objects. If, however, observations show that such fluctuations do not exist, it would not be easy to choose between the following explanations: (i) superclusters do not exist; (ii) superclusters do exist, but (a) they have sizes and density distributions which give rise to fluctuations considerably weaker than the above estimate; or (b) the X-ray background originates at large redshifts; or (c) it originates at small redshifts in objects smoothly distributed throughout the universe; or (d) it originates in small redshift objects which are all supercluster members, and which are rare (see the next section).

Few Sources

It was assumed earlier that there are many X-ray sources in each cluster. Now the opposite assumption is made: that

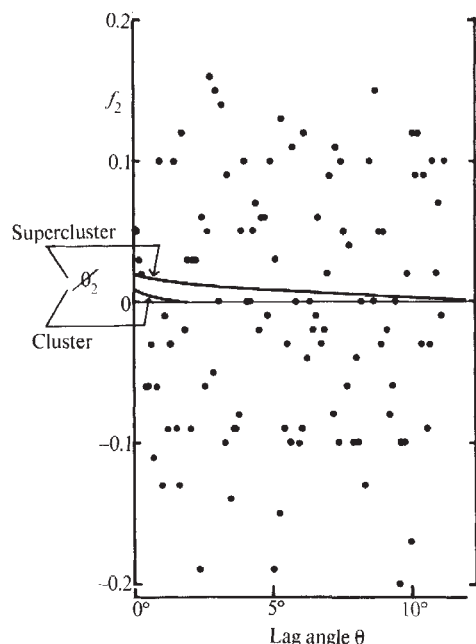


Fig. 5 Points are ACF of the data from ref. 4. The two curves are the expected mean curves of these points for the cluster ($N=100$) and supercluster models, respectively. Contrast this figure with Fig. 2 of ref. 4.

the X-ray background is integrated emission from small-redshift objects which are completely grouped into galaxy clusters, but which are sufficiently scarce that there is on average less than one source per cluster. In this case it is very unlikely that there will be two or more sources in any given cluster. Therefore cluster membership of the sources has no bearing on the calculation of the surface brightness of the sky; the sources are effectively distributed at random throughout the universe. This random distribution of sources will give rise to a broad spectrum of fluctuations, covering a wide range of

angular sizes. But no special fluctuations with the particular scale size expected for clusters of sources will be generated.

For $n=0.1$ galaxies Mpc^{-3} there are about 3×10^{10} galaxies in the sphere of radius $R_H=4,000$ Mpc centred on the Earth. If the mean number of galaxies per cluster is $30 \leq N \leq 300$, there are about 10^8 or 10^9 clusters in the sphere representing the universe. The condition that cluster-sized brightness fluctuations exist is that the number of sources in the universe be greater than this, in order that there be more sources than one in each cluster. The present observational lower limit to the number of sources in the universe⁴ is about 4×10^6 . Therefore it is quite possible that there are too few sources per cluster for cluster-sized fluctuations to be generated. The proposed superclusters contain about ten clusters, so it is also possible that even supercluster-sized brightness fluctuations do not exist.

The published observations of the X-ray background are insufficiently sensitive to detect fluctuations in the surface brightness of the sky arising from cluster or supercluster membership of the sources giving rise to the background. Even if improved observations fail to find evidence for fluctuations from clustering or superclustering, it might be that there are too few sources in the universe for the clustering to be manifested. It would not be safe to conclude either that the X-ray background originates at large redshifts, or that it originates in small-redshift objects which are not grouped into galaxy clusters.

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What Cooks with Solar Neutrinos?

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Two desperate explanations of the solar neutrino puzzle are proposed, one involving experimental nuclear physics and the other theoretical solar structure and evolution.

THE question posed in the title is not entirely facetious. "Much ado about nothing" may well be the appropriate way to describe the search for solar neutrinos, but the "ado" has been exciting and the "nothing" now poses serious problems in physics and astronomy. Seventeen years ago Davis¹ first used the beautiful technique of Pontecorvo and Alvarez, based on the reaction $^{37}\text{Cl}(\nu, e^-)^{37}\text{Ar}$, to place an upper limit on the solar neutrino flux. Not long afterward high hopes were raised involving ^8B neutrinos² with their high energy and relatively large cross section for capture³. It is now clear that

hope springs eternal but not necessarily with good reason in retrospect. Davis *et al.*⁴ have increased the sensitivity of the ^{37}Cl - ^{37}Ar technique and have shown that the detectable neutrino flux is almost one order of magnitude less than that expected from several erudite theoretical calculations^{5,6}. Here I discuss two desperate explanations for the lack of solar neutrinos, one involving experimental nuclear physics, and the other theoretical solar structure and evolution. The question is—what special nuclear cookery generates the real Sun's luminous energy (in contrast to the energy generation in theoretical models) without the production of neutrinos detectable by Davis? The neutrino threshold energy in his observations is $E_\nu=0.814$ MeV.

Experimental Physics

The reaction $^3\text{He}(^3\text{He}, 2p)^4\text{He}$, which competes with the production of ^7Be and ^8B through $^3\text{He}(\alpha, \gamma)^7\text{Be}$ and $^7\text{Be}(p, \gamma)^8\text{B}$, may be resonant at some energy below the lowest energy (90 keV) at which observations have been made⁷. If the

resonance has the appropriate characteristics it could markedly reduce the production of ${}^7\text{Be}$ ($E_\nu = 0.862$ MeV) and ${}^8\text{B}$ ($E_\nu \leq 15$ MeV). Neutrinos from these two nuclei account for $\sim 90\%$ of the counting rate now predicted in the Davis observations. The remainder come from the ${}^1\text{H}(\text{pe}^-, \nu){}^2\text{D}$ reaction with $E_\nu = 1.44$ MeV and the CN-cycle with $E_\nu({}^{13}\text{N}) \leq 1.20$ MeV and $E_\nu({}^{15}\text{O}) \leq 1.74$ MeV. The neutrinos from ${}^1\text{H}(\text{p}, \text{e}^+ \nu){}^2\text{D}$ with $E_\nu \leq 0.42$ MeV are not detectable by ${}^{37}\text{Cl}(\nu, \text{e}^-){}^{37}\text{Ar}$.

In some rudimentary calculations I have assigned optimum properties to this proposed resonance. The resonant energy, $E_r = 20$ keV, and the width, $\Gamma_r = 10$ keV, were chosen to match the most effective energy and the effective interval in energy for ${}^3\text{He} + {}^3\text{He}$ in the Sun. The narrow width chosen also guarantees that the resonance is not detectable at the lowest energies of measurement on ${}^3\text{He}({}^3\text{He}, 2\text{p}){}^4\text{He}$. The spin and parity were taken to be 0^+ so that an $l=0$ interaction was possible and the dimensionless reduced width in the incoming channel was taken to be unity, yielding $\Gamma({}^3\text{He} + {}^3\text{He}) \approx 10^{-6}$ eV at resonance. The cross section at resonance is $\sim 10^{-8}$ barn which is $\sim 10^4$ times that extrapolated without resonance from the low energy measurements⁷. Using a stellar evolution programme developed by P. P. Eggleton, D. N. Schramm has included the necessary nuclear reactions for the Sun and has shown that even for an increase of only 10^2 in the ${}^3\text{He}({}^3\text{He}, 2\text{p}){}^4\text{He}$ reaction rate the detectable neutrino flux would be below the Davis value. It seems necessary to investigate this possibility experimentally because it now constitutes a loophole in the logic of solar nuclear physics, but what a let down it will be if this simple explanation turns out to be the case.

Several words of caution are in order for those experimental nuclear physicists who may wish to look for the excited state in ${}^6\text{Be}$ corresponding to this resonance. At first sight it seems improbable for ${}^6\text{Be}$ to have a narrow state at ~ 11.5 MeV excitation. Present evidence indicates that there may be excited states in ${}^6\text{He}$ at 13.4 (2.1) and 15.3 (3.0) MeV (ref. 8) and a state in ${}^6\text{Li}$ at 15.8 (0.0) MeV (ref. 9). (The energies in parentheses are the values above threshold for ${}^3\text{H} + {}^3\text{H} \rightarrow {}^6\text{He}$ or ${}^3\text{He} + {}^3\text{H} \rightarrow {}^6\text{Li}$.) ${}^6\text{He}$ and ${}^6\text{Li}$ are the other two nuclei in the isospin triplet for mass 6, but there are no angular-momentum, parity, or isotopic-spin selection rules to inhibit breakup of any state in ${}^6\text{Be}$, produced by ${}^3\text{He} + {}^3\text{He}$, into $\alpha + 2\text{p}$ either through ${}^5\text{Li} + \text{p}$ or the di-proton + α . So the width ought to be at least 1 MeV unless the overlap of the intermediate and final configurations is unusually low. Indeed, there are relatively narrow states in ${}^5\text{He}$ and ${}^5\text{Li}$ just above threshold for ${}^3\text{H} + {}^2\text{D}$ and ${}^3\text{He} + {}^2\text{D}$, respectively, which have small dimensionless reduced widths ($\sim 1\%$) for breakup into a nucleon plus alpha particle. These states are almost pure mass 3-mass 2 clusters. Perhaps the same thing happens in ${}^3\text{He} + {}^3\text{He}$. Another example of a state just above threshold is the ground state of ${}^8\text{Be}$ which is most probably an almost pure ${}^4\text{He} + {}^4\text{He}$ cluster.

The relevant range of excitation in ${}^6\text{Be}$ has been studied¹⁰ using the reaction ${}^6\text{Li}({}^3\text{He}, \text{t}){}^6\text{Be}$ at ~ 50 keV resolution but with no indication of resonance. Of course, if there were a wide state it would have shown up in the ${}^3\text{He}({}^3\text{He}, 2\text{p}){}^4\text{He}$ measurements. Other reactions which seem "promising" are ${}^7\text{Be}({}^3\text{He}, \alpha){}^6\text{Be}$ and ${}^6\text{Li}(\text{p}, \text{n}){}^6\text{Be}$. Resolution of the order of 10 keV in incident plus outgoing energy is required. This will be difficult. Those who are determined can be encouraged, however, by the old adage "if it were easy, someone else would have done it".

Theoretical Considerations

In speculating on possible difficulties in our present understanding of solar structure and evolution it must be remembered that the diffusion time for radiation from the centre of the Sun to the surface^{11,12} is of the order of the Kelvin-Helmholtz time:

$$\tau_{\text{KH}} \sim \frac{GM^2_{\odot}}{R_{\odot}L_{\odot}} \sim 3 \times 10^7 \text{ yr}$$

So changes in solar luminosity will be delayed by the order of τ_{KH} if variations in solar energy generation occur over intervals less than τ_{KH} . On the other hand, the neutrino flux at Earth, delayed by only eight minutes in transit from the Sun, will accurately monitor any changes in the nuclear processes responsible for energy generation in the solar interior. One might take a determined stance in this matter. Equating the energy generation to the present solar luminosity leads to a calculated central temperature of 15×10^6 K. As I will show, the low neutrino flux implies an upper limit $\sim 10\%$ lower than this. Therefore, the present energy generation does not match the present luminosity and the Sun is in a transient phase. At least some thought should be given to the matter.

An attractive possibility, at first sight, is a periodic variation in the Sun's central temperature with period $< \tau_{\text{KH}}$ so that the solar luminosity averages out to the present value, and Davis (and the rest of us) are unfortunately living at a time of minimum temperature with relatively few neutrinos from ${}^7\text{Be}$ and ${}^8\text{B}$. Coulomb barrier factors make the production of ${}^7\text{Be}$ and ${}^8\text{B}$ very temperature sensitive, $\propto T^{11}$ and T^{25} , respectively, when all factors are taken into account². Such variations, however, will rapidly damp out and I am unable to suggest a sufficient driving force.

On the other hand, a sudden change in solar structure within the past 10^7 yr or so is not so easily rejected. It is well known that the radiative interior of the Sun is not all that far from convective instability. Let the Sun evolve as the pundits claim (and with no resonance in ${}^3\text{He} + {}^3\text{He}$) for most of its lifetime, $\sim 4.7 \times 10^9$ yr, so that, as a result of non-convective hydrogen burning via the pp-chain, the central mass fractions are, for example, $X = 0.415$, $Y = 0.570$ and $Z = 0.015$ whereas the outer composition is still characterized by the primordial values $X = 0.735$, $Y = 0.250$ and $Z = 0.015$. Then, some time within the past τ_{KH} yr or so, for reasons unspecified in detail, let convective instability set in over all of the Sun so that the mass fractions become $X = 0.735 - 0.04 = 0.695$, $Y = 0.250 + 0.04 = 0.290$, $Z = 0.015$, assuming that the mass fraction of hydrogen converted to helium over the previous life of the Sun is 0.04, ensuring approximately constant luminosity over the age of the Sun. There will be a decrease in the mean molecular weight of the central material and this decrease will be substantial; $\mu = 0.79$ before convection and $\mu = 0.62$ afterward. Of course, the decrease will not be so drastic if only part of the solar interior becomes convective.

The effects accompanying and following the onset of convective mixing are difficult to predict. I speculate that the run of pressure, p , and density, ρ , with radius might not necessarily change so that $p/\rho c^2 = \int T/\mu c^2$ remains constant at any given radius and T decreases proportionately with μ , to 79% of its value before mixing, in the extreme case. A less drastic speculation is that the time for the convective mixing and the resulting structural reaction is short enough that little energy is lost by the Sun compared with its internal store, that is, the binding energy remains constant. At the centre, c , of polytropic structures with index n

$$(p/\rho c^2)_c = (\int T/\mu c^2)_c = \frac{2R_n(5-n)}{3(n+1)M_n} \varepsilon_B$$

where $\varepsilon_B = E_B/Mc^2$ is the fractional binding energy and R_n and M_n are the constants of integration corresponding to radius and mass for the Lane-Emden equation. The coefficient of ε_B depends only slightly on n , increasing by $\sim 10\%$ from $n = 3.5$ (a good average for the radiative Sun) to $n = 1.5$ (a good average for a convective Sun). Again, $T_c \propto \mu$ so that T_c will drop as a result of the mixing in of fresh hydrogen. This drop is much more than enough to make up for the increase in the reaction rates due to the increased abundance of hydrogen. Given the temperature sensitivity for the ${}^7\text{Be}$ and ${}^8\text{B}$ production previously quoted, simple calculations show that the neutrino flux drops well below the observed upper limit. This has been confirmed in detailed model calculations by R. Rood at the California Institute of Technology (personal com-

munication). That is the case even if fractional mixing has occurred several times in the past; so that the recent past need not be all that special. The situation is somewhat similar to that for the zero-age main-sequence Sun. On the other hand, previous calculations on mixing¹³ are irrelevant to the present problem because it was assumed that the energy generation remained in balance with the present luminosity of the Sun. I have no answer for those who worry that the Sun will not become a Red Giant except to comment that perhaps the Sun is not so common a star after all.

The energy generation in the solar interior $\propto (\rho X^2 T^5)_c$ will also decrease somewhat but the luminosity will decrease to a corresponding lower value only over τ_{KH} yr or some small multiple thereof. Has the Sun been cooling off over the past 30–100 m.y. and has the Earth been affected? One thinks immediately of glaciation in the very recent geological past (~ 1 m.y.)¹⁴ and of the decrease in oxygen isotope temperatures between “32 and 1 m.y. ago”¹⁵. At least the geological record does not rule out this second desperate explanation for the present low neutrino flux but it would seem well at this point to rest the case without further ado.

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Altered Enzymes in Ageing Human Fibroblasts

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Diploid human fibroblasts accumulate heat labile enzymes during the final stages of their life-span in culture. The RNA base analogue 5-FU induces premature senescence, which is preceded by the appearance of altered enzyme. These observations support Orgel's "error catastrophe" theory of ageing.

Hayflick and Moorehead^{1,2} first demonstrated that diploid human fibroblasts have a limited life-span in culture, an observation which has subsequently been confirmed in many other laboratories. It has also been clearly shown that cultured fibroblasts from young donors have a longer life-span than those from old individuals^{2,3}. A rare inherited condition in man, Werner's syndrome, leads to premature ageing with clinical symptoms very similar to those in the normal age range⁴. Martin *et al.*⁵ have demonstrated that fibroblasts from four patients with Werner's syndrome could be subcultured only a few times before the cells died. These observations strongly suggest that the mortality of cultured fibroblasts is in some way related to the mortality of the individual. They do not show that the cause of fibroblast ageing is necessarily the main cause of normal ageing of the whole organism.

Clonal senescence has also been described and examined in detail in certain microorganisms, particularly fungi.

Evidence has been obtained with certain mutant strains of *Neurospora* that senescence is accompanied by the synthesis of defective protein molecules, with a rapid increase in their production just before growth ceases^{5,6}.

These results provide evidence for Orgel's theory of ageing⁷, which suggests that one cause of ageing could be the accumulation of errors in protein synthesis, leading to a gradual but irreversible breakdown in the accuracy of the protein synthesizing machinery and finally to a lethal "error catastrophe". We demonstrate here that the clonal senescence of human fibroblasts is accompanied by the appearance of a proportion of heat labile enzyme. Moreover, by this biochemical test we can predict when a culture will become visibly senescent.

The Growth of MRC-5

We used the male foetal lung fibroblast strain MRC-5, which has been fully characterized⁸. It grows somewhat faster than the better known WI-38 and its life-span in culture appears to be longer. Most commonly, cells became visibly senescent after about fifty subcultures and ceased growth after 60–65 subcultures. Cells were grown at 37° C in Eagle's basal medium containing 10% foetal calf serum and 200 units of penicillin and 200 µg/ml. of streptomycin. Cultures were trypsinized and split 1:2 twice weekly. If a culture had not become confluent after 3 or 4 days, the medium was changed. The age of a culture was measured by its passage number rather than by chronological time.

Senescent cultures have slow growing cells of very variable size which often have granular cytoplasm. They tend not

to line themselves up in the normal parallel arrays and do not reach the usual cell density when confluent. Part of the culture may achieve confluency, whereas other parts remain patchy. Very little is known about the viability of cells in fibroblast cultures of increasing age. We presume that there is considerable variation in the degree of senescence of cells in any ageing culture and that the more healthy cells will continually be selected from a heterogeneous population. It is often observed that a culture appears to have stopped growing, but that after one or more changes of medium it gradually becomes confluent and can then be subcultured several more times before growth finally ceases. In other words, a culture does not necessarily proceed inexorably and steadily to senescence and death; it may well pass through phases of increasingly retarded growth with intervening phases of more normal growth.

Heat Labile Enzyme

Orgel's theory predicts that in ageing cells there should be a mixture of normal and abnormal protein and that the abnormal fraction should itself be heterogeneous, as the errors introduced would be a random collection of amino-acid substitutions. In such a situation, standard methods for detecting enzyme heterogeneity, such as starch or polyacrylamide gel electrophoresis, are very unlikely to detect the altered fraction, which would be dispersed in many positions in the gel. It has been shown that random amino-acid substitutions frequently make an enzyme easily denaturable by heat. For example, Langridge^{9,10} studied missense mutations of β -galactosidase where serine or glutamine was substituted for ochre or amber mutations. Of fifty-two serine substituted mutations in different positions in the protein, sixteen had a half-life at the inactivating temperature at least ten times less than the wild type enzyme, and a further fifteen had a half-life at least two times less. Of fifty-six naturally occurring human variants of glucose-6-phosphate dehydrogenase, twenty-seven are listed as being heat labile¹¹.

In ageing *Neurospora* cultures all the glutamic dehydrogenase became heat labile⁶, but it would be surprising if mammalian cells were as tolerant of abnormal protein molecules. In order to detect a few per cent of heat labile enzyme, it is necessary to use an enzyme which is normally

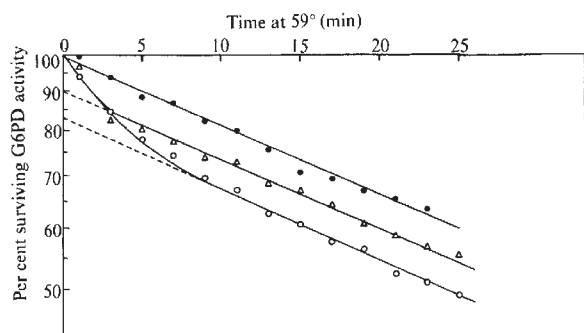


Fig. 1 Inactivation at 59° of glucose-6-phosphate dehydrogenase obtained from young, middle aged and senescent cells. Confluent cultures were rinsed with phosphate buffer saline (Dulbecco A) and harvested with trypsin. The cells were washed twice with 0.005 M Tris/HCl in 0.15 M NaCl (pH 7.4). The pellet was resuspended in 0.05 M Tris/HCl (pH 8.0) containing 10^{-3} M mercaptoethanol, 10^{-3} M EDTA, 4×10^{-3} M ϵ -amino-n-caproic acid and 10^{-3} M NADP, and the cells disrupted by sonication. Cell debris were removed by centrifuging at 18,000 r.p.m. at 4° C for 30 min. The supernatant (approximately 0.6 ml. per 20-oz. culture bottle) was assayed for G6PD activity. The assay consisted of 0.02 ml. enzyme and 0.78 ml. 0.25 M Tris/HCl buffer (pH 8.6) containing 9.5×10^{-3} M glucose-6-phosphate (Na salt), 3.2×10^{-4} M NADP and 1.9×10^{-2} M $MgCl_2$. The rate of increase in absorbance at 340 nm was measured with a Pye Unicam SP1800 spectrophotometer, each assay being run for 1 min. The extraction and assay procedures are based on those of Steele¹⁶.
●, Passage 22; △, passage 48; ○, passage 61.

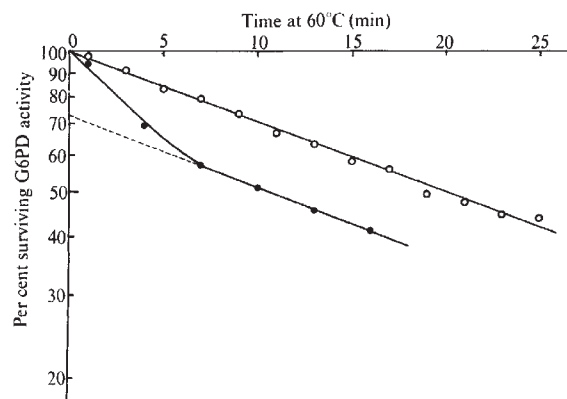


Fig. 2 Inactivation at 60° of G6PD obtained from young and senescent cultures. ○, Passage 23; ●, passage 61.

homogeneous and which therefore loses activity exponentially during heat treatment. (Mixtures of isoenzymes would not be expected to decay exponentially.) It is also essential to have an accurate assay. Of eight enzymes we have so far examined in crude extracts obtained from fibroblasts, only two have fulfilled these criteria: glucose-6-phosphate dehydrogenase (EC 1.1.1.49) and 6-phosphogluconate dehydrogenase (EC 1.1.1.44).

Glucose-6-phosphate Dehydrogenase

G6PD consists of a single polypeptide chain coded for by a gene on the X chromosome^{12,13}. The enzyme is in a stable and active multimeric form only in the presence of NADP^{14,15}. Activity is measured by the rate of formation of NADPH₂ in the presence of the substrate (details of cell free extracts and the enzyme assay are given in the legend to Fig. 1). We carefully checked that with the assay conditions used, none of the increase in absorption at 340 nm is due to the activity of 6-phosphogluconate dehydrogenase¹⁷. To determine the fraction of heat labile enzyme, it is essential to measure as accurately as possible the activity before heat treatment. This initial measurement was carried out in triplicate; the extract was then heated to 59° C or 60° C and samples removed and assayed at 2 min intervals. Almost all of our determinations are based on 12–15 assays and we believe that the estimate of the heat labile fraction of enzyme is correct to within 3–5% of the total initial activity. When both inactivating temperatures were used, the difference in the estimate of the heat labile fraction was never greater than this. The inactivation at 59° C of G6PD in three extracts from cultures of different age is shown in Fig. 1. The enzyme from the passage 22 cells had no heat labile fraction. The passage 48 cells showed no morphological symptoms of senescence, but by passage 51 they were visibly senescent. The passage 61 cells showed all the symptoms of senescence and the line died out at passage 63. More often the inactivation of enzyme at 60° C was followed: examples are shown in Fig. 2. The passage 61 cells were from the very last subculture and insufficient extract was available for the normal number of assays. Nevertheless, it is quite clear that these senescent cells contain about 27% heat labile altered enzyme, the largest proportion we have detected. Fig. 3 summarizes a large number of results obtained with extracts from cells of different ages. In general, young cells have about 5% heat labile enzyme, whereas old cells have 15–25%. The variation in the amount of heat labile enzyme in old cells is not due primarily to inaccuracy in measurement, but rather to the considerable variation in the degree of senescence which has already been referred to. The closed circles in Fig. 3 are the successive determinations of the proportion of heat labile enzyme in a single line cultured from the period before senescence until

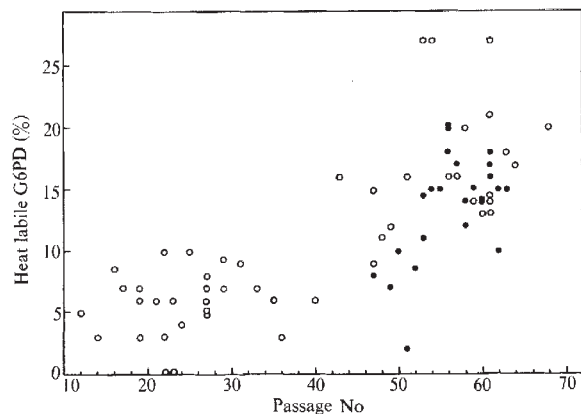


Fig. 3 Estimated proportion of heat labile G6PD in cultures of different ages. Each point is based on an experiment like those in Figs. 1 and 2. ○, Measurements with assorted cell lines of different age; ●, successive measurements on one cell line which finally died at passage 63. More than one point at the same passage number indicates either that both 59° C and 60° C were used to determine the heat labile fraction, and/or that cultures of the same passage number were examined on different days.

death. (More than one point at each passage number indicates that the enzyme was examined at both 59° C and 69° C, or that cells of the same passage number were examined before and three or more days after a change of medium.) In this experiment and on other occasions it was possible to predict when a culture will become visibly senescent by the appearance of significantly more than 10% of heat sensitive enzyme. The fraction of heat labile enzyme then rapidly increased to passage 56 and thereafter fluctuated. Orgel's theory predicts that the amount of altered protein should rise rapidly immediately before death; but there are two reasons why this might not be observed. One which has already been discussed is the heterogeneity of cultures due to the continual selection of the healthiest cells. The other is that the heat labile enzyme we are looking at is likely to have a shorter half-life *in vivo* than normal enzyme¹⁸. As cells become senescent, they grow much more slowly and make less protein, so altered enzyme will tend to disappear from such cultures more rapidly than normal enzyme.

Altered Substrate Specificity

G6PD can act on the analogue substrate 2-deoxyglucose-6-phosphate with low efficiency, the rate of the reaction being about 5% of that with the normal substrate. A number of naturally occurring mutant forms of the enzyme can use dG6P with very much higher efficiency¹¹, and errors in protein synthesis should also produce molecules with this property. But an amino-acid substitution which alters the substrate specificity would also in many cases make the enzyme heat labile. One would therefore expect a greater

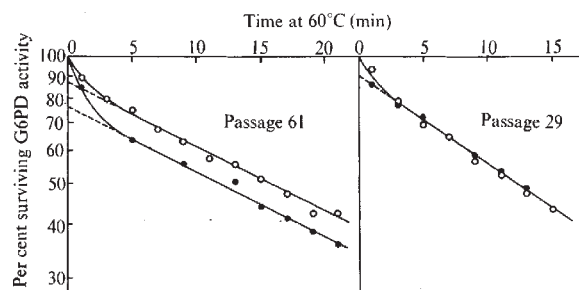


Fig. 4 Inactivation at 60° of G6PD with the normal substrate (○) or the analogue substrate, 2-deoxyglucose-6-phosphate (●), using extracts from young and senescent cultures. In the assay mix the concentration of dG6P (Na salt) was 10^{-2} M.

initial heat inactivation of the enzyme reacting with dG6P than with the normal substrate. After the altered molecules had decayed, the ratio of activities should remain constant. With cell free extracts from old cells this is precisely what we observed (Fig. 4). With young cells, the activity with the two substrates decays throughout the heat treatment at the same rate, even though a small proportion of heat labile enzyme is present. This strongly suggests that the heat labile enzyme in old cells is qualitatively different from that observed in extracts from young cells.

6-Phosphogluconate Dehydrogenase

This enzyme, 6PGD, is in the same pathway as G6PD. It, too, is measured by the rate of formation of NADPH₂ in the presence of the substrate, but the amount of activity is only about one tenth that of G6PD, and at the inactivating temperature used, 52° C, there is initially a slight increase of enzyme activity. For both these reasons the determination of the fraction of heat labile enzyme is less accurate than with G6PD, perhaps to within 5–10% of the total initial activity. We do not know whether young cells have a small fraction of heat labile enzyme, but senescent cells may contain at least 25%. Fig. 5 shows experiments which demonstrate this. If all the proteins in senescent cells are being synthesized inaccurately there would, of course, be a correlation between the proportion of heat labile G6PD and 6PGD. Fig. 6 shows that such a correlation can be demonstrated.

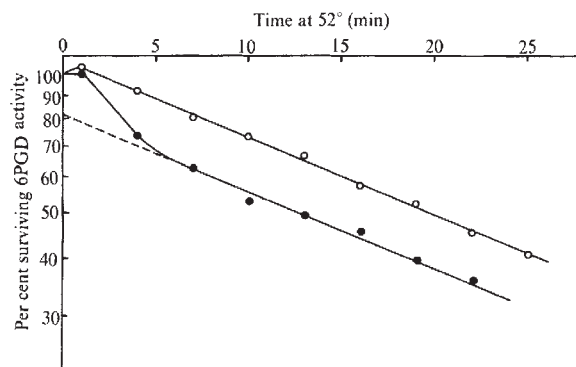


Fig. 5 Inactivation at 52° of 6-phosphogluconate dehydrogenase obtained from young and senescent cultures. The substrate was 7×10^{-4} M 6-phosphogluconic acid (Ba salt), otherwise the cell free extract and enzyme assay was the same as with G6PD. ○, Passage 25; ●, passage 64.

Experiments with 5-Fluorouracil

If ageing is due to the accumulation of errors in protein synthesis, then agents which directly or indirectly introduce mistakes in transcription or translation should induce premature senescence. It has been previously shown that amino-acid analogues can have this effect in *Drosophila*¹⁹, *Podospira*⁵ and *Neurospora*⁶. The base analogue 5-fluorouracil (FU) is incorporated into RNA and introduces errors in transcription or translation²⁰, and it has been shown in *E. coli*²¹ and *U. maydis*²² that heat labile enzyme is synthesized under these circumstances. We have examined the effects of growing fibroblasts in very low concentrations of FU. Concentrations lower than $1 \mu\text{g ml}^{-1}$ FU (in the form of the Tris aminomethane salt) allow cell division, and a growth experiment with $0.25 \mu\text{g ml}^{-1}$ is shown in Fig. 7. At each subculture the trypsinized cells were counted and split 1:4. Initially the cells were at passage 22: they grew vigorously in the medium containing FU for about ten cell doublings, finally reaching the equivalent of passage 32 before growth ceased. The control eventually reached passage 67. In this and other experiments the cells initially appeared to

be normal, but after subculturing a few times the characteristic morphological symptoms of senescence appeared. It is, in fact, hard to distinguish cells grown for a few generations in FU from those which have become senescent in its absence.

Table 1 Estimated Proportion of Heat Labile G6PD in Fibroblasts grown in Medium containing 5-Fluorouracil and in Control Cultures

	Passage No.	Days of growth	Heat labile enzyme (%) With FU	Control
Experiment 1 0.25 $\mu\text{g ml}^{-1}$ FU	19	3	11	10
	21	10	10	—
	23	17	7	—
	25	24	8	8
	27	32	15	8
	29*	39	17	6
Experiment 2 0.125 $\mu\text{g ml}^{-1}$ FU	17	5	5	7
	19	9	9	7
	21	13	6	6
	23	16	10	6
	25	20	20	10
	27	27	21	8

* FU treated cells ceased growth at passage 31.

It might be expected that the addition of FU to cultures would immediately result in the synthesis of altered proteins, but no greater proportion of heat labile G6PD was initially observed in treated cells than in untreated controls. After a few generations of growth, however, heat labile enzyme appeared and the growth rate then fell. We have not followed the changes in the fraction of heat labile enzyme in cultures treated with FU in full detail, but the results of two preliminary experiments are summarized in Table 1. In both cases, a proportion of heat labile G6PD significantly higher than in the controls appeared after ten generations of growth in FU. The results strongly suggest that the analogue is having an indirect effect on the formation of altered enzyme, and this could be due to the development of an error catastrophe in protein synthesis.

Nature of the Altered Enzyme

The altered proteins we have observed in extracts from senescent cultures need not necessarily have been derived from faulty transcription or translation. It is possible that the heat labile enzymes are formed as a result of somatic mutation. In this case a minority of cells would be synthesizing only altered enzyme and the rest normal enzyme. We have no method at the moment for detecting single cells or colonies which produce entirely heat labile enzyme, but it is hard to believe that at least 25% of the population is

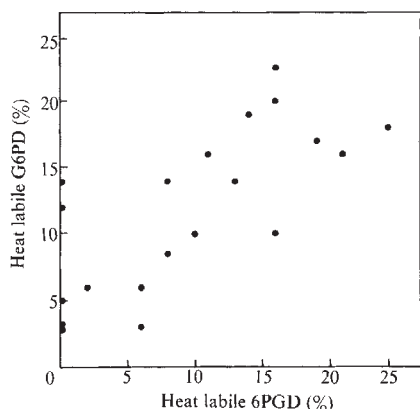


Fig. 6 Correlation between the estimated amount of heat labile G6PD and 6PGD in cultures of various ages. Each point is based on two experiments like those in Figs. 1, 2 and 5, carried out with the same cell free extract.

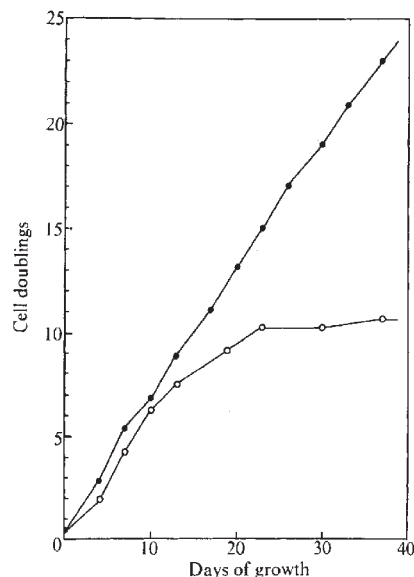


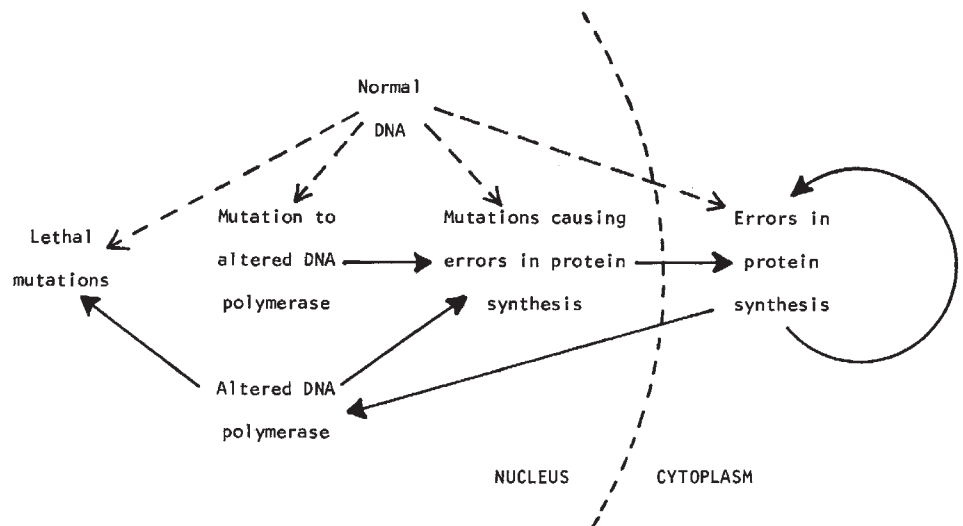
Fig. 7 Growth of MRC-5 cells in normal medium and in medium supplemented with 0.25 $\mu\text{g/ml}$ 5-fluorouracil (in the form of the Tris aminomethane salt). The experiment was started with passage 22 cells. At each subculture the cells were trypsinized, suspended in 4 ml. medium and counted with a haemocytometer. One ml. of suspension was added to each of two 8-oz. medical flats containing 20 ml. medium. When the FU treated cells became senescent, several bottles were inoculated and counted at intervals. None became confluent. ●, Control; ○, +5FU.

mutant for the genes coding for G6PD and 6PGD, for then every senescent cell would contain extremely large numbers of mutations, and many of these would be lethal and prevent growth long before the culture finally dies. Furthermore, if somatic mutations are simply accumulating in time, one would expect a gradual steady increase in the proportion of heat sensitive enzyme throughout the life-span, rather than the rapid rise during the last fifteen passages which is observed. There is, however, preliminary evidence that mutations do become frequent in senescent cultures (L. K. Hohmann and S. J. Fulder, personal communication) and, as we shall see, they may well play a role in ageing—but the rate of their occurrence is insufficient to account for the heat labile enzymes we observe.

Another possibility is that post-synthetic alteration of the enzyme might occur, for example, by the loss of amide groups; or they may become partially denatured. If protein synthesis or turnover is slower in old cells, there may be more time for the enzymes to be altered and thus a higher proportion of heat sensitive molecules would be seen. We have attempted to test this by depriving young cultures of serum, or by adding cycloheximide, and then preparing cell free extracts 2–3 days later. We detected no increase in heat labile enzyme; nor does prolonged storage at 5° C have this effect. The fact that two enzymes behave very similarly during ageing strongly argues against changes in heat lability as a result of post-synthetic modification. It is also unlikely that post-synthetic changes of this type alter the substrate specificity of the heat labile fraction (Fig. 4). Finally, it is hard to see how only prolonged growth in medium containing FU should have this effect. The most likely explanation of our results is that the heat sensitive enzymes are formed as a result of the breakdown of the fidelity of protein synthesis and the misincorporation of amino-acids into polypeptide chains.

Our results are in agreement with some other studies. Lewis and Tarrant (in preparation) compared the amount of lactic dehydrogenase enzyme activity with enzyme cross-reacting material (CRM) in MRC-5 cells throughout the life-span. A few passages before death, the enzyme/CRM

Fig. 8 Schema for senescence.



ratio decreased rapidly, suggesting that a considerable proportion of inactive enzyme was being synthesized. Wang *et al.*²³ found that phosphatases from late passage WI-38 fibroblasts responded in a different way to inhibitors from enzymes from young cells, and concluded that altered enzymes may be present.

If 25% heat labile G6PD is due to misincorporation of amino-acids, it is interesting to estimate what the error frequency might be. The molecular weight of the polypeptide chain is about 43,000 (ref. 14), perhaps 450 amino-acids. If we assume that most random amino-acid substitutions do not inactivate the enzyme and that about half of them make the enzyme heat labile (see above), then approximately one polypeptide in two contains an incorrect amino-acid—a misincorporation frequency of 10^{-3} . (This calculation leaves aside the question of the heat stability of enzyme molecules containing a mixture of normal and altered subunits.) Young fibroblasts usually contain about 5% heat labile G6PD, which could be due to an error frequency of about 2×10^{-4} , but the experiments on substrate specificity make it more likely that this small proportion of heat labile enzyme is formed by post-synthetic modification in all populations. Lofffield²⁴ estimated that the error frequency in the synthesis of chicken ovalbumin could not be higher than 3×10^{-4} per amino-acid residue.

Causes of Cellular Death

If altered proteins are synthesized in senescent fibroblasts, this does not necessarily mean that the cells are finally killed by the presence of an error catastrophe in protein synthesis. In previous studies of ageing with *Neurospora*⁶, large numbers of mutations were detected in a strain, *leu-5*, which has an altered activating enzyme and synthesizes defective proteins, and the question arose whether these mutations were one of the causes of the ageing which is observed in this strain. It has also been shown that amino-acid analogues and FU are mutagenic²⁵, although it is not easy to see that they could act directly on DNA. It was therefore suggested that in all these cases mutations arose because faulty DNA polymerase molecules cause errors in genetic replication, as is known to be the case in bacteriophage T4²⁵. Fig. 8 illustrates some general principles which may underlie the overall mechanism of ageing.

This schema will account for the genetic control of ageing, as both the initial error frequency in protein synthesis and the mutation rate would be strongly influenced by genetic factors. The less common the events marked by dashed arrows are, the greater the stability of the cell and its life-span will be. If either an Orgelian increase in protein errors begins to develop or a chance mutation reduces the accuracy of protein synthesis, the cell will move into a metastable

condition in which any of the events marked by solid arrows can occur with increasing frequency. In the inevitable and irreversible change to a completely unstable state, the altered DNA polymerase may play a crucial role and the cell could eventually be killed either by an error catastrophe or by lethal mutation.

With regard to the initiating event, experiments on clonal ageing in fungi^{26,27} and amoeba²⁸ show that the determinant of senescence is transmitted by the cytoplasm, not the nucleus, suggesting that slow buildup in errors in protein synthesis may be the primary cause of ageing⁵. It would, however, be very surprising if different organisms, or even different types of cell in the same organism, did not vary considerably in the relative importance of the nuclear and cytoplasmic changes which lead to the initiation and development of senescence.

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LETTERS TO NATURE

PHYSICAL SCIENCES

Galactic Gamma-radiation between 200 MeV and 10 GeV

THE presence of γ -ray emission from the Galaxy at energies above 100 MeV was first reported by Clark *et al.*^{1,2}. It has not yet been established whether most of this radiation is a diffuse line flux from the galactic disk or if it arises from a number of weak point sources, and the production mechanism for each case remains poorly understood. In principle, the uncertainty about the production mechanism can be resolved by examining the energy spectrum of the γ -rays with high angular resolution detectors. Such attempts have been made in the energy range 30–300 MeV (refs. 3–6), using multiplate spark chambers. Our experiment was undertaken to study the high energy part of the spectrum between 200 MeV and 10 GeV, using balloon-borne detectors.

In the detector (Fig. 1) half a radiation length of lead was used to convert the incoming γ -rays into e^+e^- pairs. The electronic logic for a γ -ray was ABCD, and all events of this type which exceeded the discriminator level of D (> 50 MeV) triggered the spark chamber gaps S₁ and S₂ and enabled the pulse height information from D to be read out. The spark chamber was of the multi-wire grid type. The spark location was determined using the magnetostrictive wire technique and the coordinates were read out in a digital

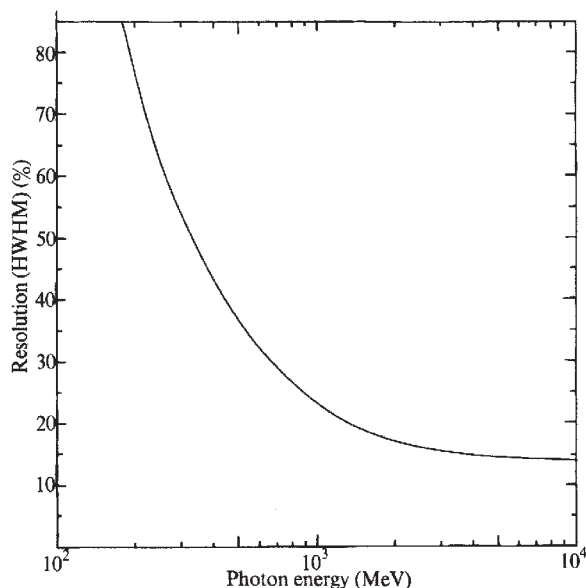


Fig. 2 Energy resolution of the detector for γ -rays.

form. The angular resolution of the detector was limited by multiple Coulomb scattering in the converter material at the lower energy end of the spectrum and by the spark chamber geometry at the higher energies; it ranged from $\pm 5^\circ$ at 200 MeV to $\pm 3^\circ$ at 1 GeV and above.

The energy of the incoming γ -ray was measured in element D, a 10 radiation length deep cylindrical block of Schott SF6 lead-glass, coupled to a 5 inch diameter photomultiplier tube. The energy response of this element was measured in the 4.45 GeV NINA electron test beam at Daresbury, Lancashire, and this calibration was used to calculate the response to γ -rays. The energy resolution of the detector for γ -rays is shown in Fig. 2.

The detector had a sensitive area of 125 cm² and a geometric opening angle to the zenith of $\pm 45^\circ$. The overall detector efficiency for γ -rays was 19% at 200 MeV, rising to 21% at 1 GeV. Aspect information for each γ -ray event was determined using a three-axis flux-gate magnetometer, and cross checked using a solar sensor. Inflight calibration for the lead-glass calorimeter and the spark chamber electronics was also provided.

Two detectors of this type were flown from Lusaka, Zambia (12.5 GeV cutoff rigidity), on January 25 and February 9, 1971. Exposure at altitude was limited by the extremely high stratospheric winds (140 km h⁻¹) encountered in Zambia. The first payload reached an altitude of 4.1 mbar at 0701 UT and remained at this level for 4.5 h before radio contact was lost. The second flight reached an altitude of 11 mbar and then sank slowly because of a balloon fault; useful data were recorded only between 0604 UT and 0720 UT. Because of the high background rate of flight 2 and the uncertain atmospheric depth correction, most of the data from this flight have not been used here.

We analysed the data for arrival direction in galactic coordinates using strips 2° wide in b^{II} and strips in l^{II} of such a width as to give bins of equal solid angle normalized to $2^\circ \times 2^\circ$ on the galactic equator. After bins had been added together to make an angular sum equal to the resolving power of the telescope at various energies, the sky map was scanned for possible point sources. Only one position on the map suggests

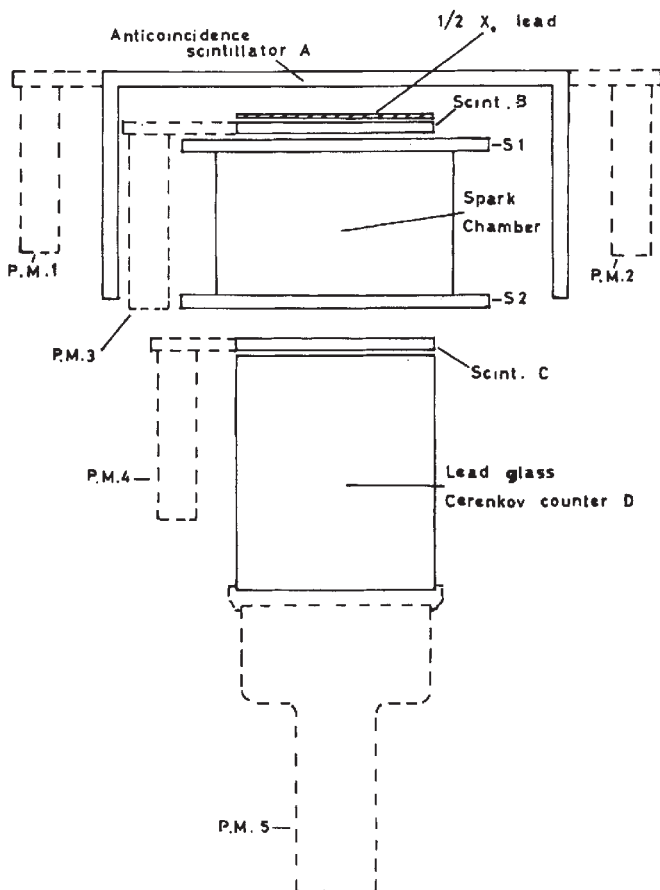


Fig. 1 Schematic diagram of the detector.

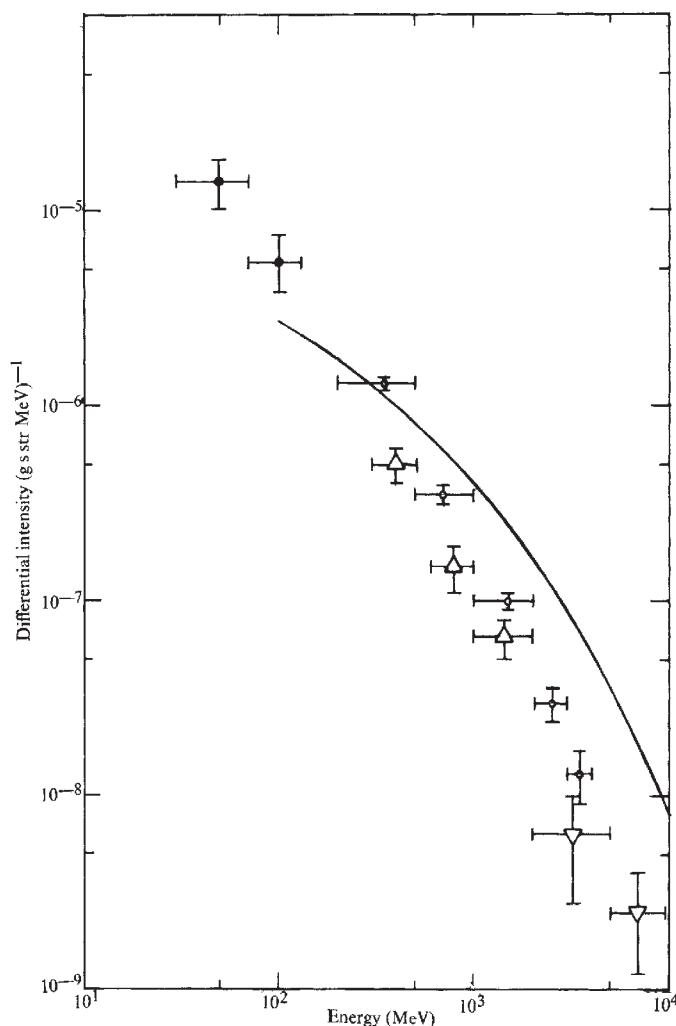


Fig. 3 Experimental and theoretical atmospheric γ -ray spectra, normalized to unit atmospheric depth. $\bullet$, Fichtel *et al.*⁹; Δ , Fazio *et al.*¹⁰; ∇ , Anand *et al.*¹¹; $\circ$, this experiment; —, Perola and Scarsi¹².

any evidence for a source. The solid-angle bin centred on the X-ray source SGR XR-2 shows an excess number of counts over the background from the surrounding region at all energies (Table 1). The most significant excess is at 1 GeV, where the chance probability of obtaining the observed 9 counts above the background of 3.6 counts is 7×10^{-3} . This evidence for a point source is marginal, however, and the results are quoted only as upper limits, following the method of Hearn⁷. The limits for SGR XR-2 at various energies are shown in Table 2 with those from other likely sources. We emphasize that if the values quoted for SGR XR-2 were positive fluxes, they would be much higher than the limit of 4×10^{-5} photons $\text{cm}^{-2} \text{s}^{-1}$ above 50 MeV established by Fichtel *et al.*⁸.

Because flight 1 had floated at a constant altitude for 4.5 h, it was possible to examine the sky map for this flight for evidence of an extended line source. The γ -ray counts were summed in 2° strips in galactic longitude (l^{II}) and the effective exposure for each strip calculated. The regions away

Table 2 Upper Limits for Possible γ -Ray Sources

Source	Energy range			
	≥ 200 MeV	≥ 500 MeV	≥ 1 GeV	≥ 2 GeV
GX 3+1				
(SGR XR-1)	7	3	3	2
SGR XR-2	8	8	6	3
OPH XR-1	7	3	2	2
GX 2+3	7	5	3	2
SER XR-2	5	4	4	2
Quiet Sun	7	5	2	1

95% confidence level flux limits ($\times 10^{-5}$ photons $\text{cm}^{-2} \text{s}^{-1}$).

Table 3 γ -Ray Counts Observed for a Galactic Line Source, for $-9^\circ < b^{\text{II}} < +9^\circ$ and $-20^\circ < l^{\text{II}} < 50^\circ$

Photon energy	Counts observed	Counts expected	Line source flux $\times 10^{-4}$ photons $\text{cm}^{-2} \text{s}^{-1} \text{rad}^{-1}$
> 200 MeV	210 ± 14	177 ± 9	2.8 ± 1.4
> 1 GeV	41 ± 6	36 ± 4	1.25^*
> 2 GeV	13 ± 4	11 ± 2	0.76^*

* 95% confidence level upper limit.

from the galactic equator were used to set a value for the atmospheric background count rate. From this we estimated the number of counts expected from the background alone in the central strip. There was an apparent flux from the galactic disk above the expected count rate and the width of this central strip running along the galactic equator was increased in 2° steps until a maximum signal to noise ratio was obtained. At our energy threshold of 200 MeV the signal from $\pm 9^\circ$ strip was two s.d. above the background, corresponding to a flux of $2.8 \pm 1.4 \times 10^{-4}$ photons $\text{cm}^{-2} \text{s}^{-1} \text{rad}^{-1}$. Because no significant signal was observed at energies greater than 1 GeV we have set upper limits. The events triggering the detector below 200 MeV and greater than 50 MeV were also inspected for a possible line source. There were 39 excess counts from a $\pm 9^\circ$ strip, in a flux of 435 counts. At approximately one s.d. this signal is of little significance. We draw no conclusions from the flux below 200 MeV because multiple scattering in the converter is large at these lower energies and can effectively increase the acceptance cone of the detector for such events. Confirmation that the detector was functioning properly and that it had been correctly calibrated was obtained by comparing our measured atmospheric background γ -ray spectrum with previously reported results⁹⁻¹¹ (Fig. 3).

Our flux values can be compared with those obtained by Clark *et al.*² of $(1.2 \pm 0.4) \times 10^{-4}$ photons $\text{cm}^{-2} \text{s}^{-1} \text{rad}^{-1}$ for $-15^\circ < b^{\text{II}} < +15^\circ$ and $-60^\circ < l^{\text{II}} < +60^\circ$, and by Fichtel *et al.*³ of $(2.0 \pm 0.6) \times 10^{-4}$ photons $\text{cm}^{-2} \text{s}^{-1} \text{rad}^{-1}$ for $-6^\circ < b^{\text{II}} < +6^\circ$ and $-39^\circ < l^{\text{II}} < +24^\circ$. Both these results are for energies greater than 100 MeV. Our data provide further evidence for the existence of a galactic line source of high energy γ -rays in the region of the galactic centre. Our results are consistent, within the errors quoted, with an extrapolation of the fluxes measured by Fichtel and Clark but disagree with the 95% confidence upper limit set by Frye *et al.*⁵ of 5×10^{-5} photons $\text{cm}^{-2} \text{s}^{-1} \text{rad}^{-1}$ for photons greater than 100 MeV.

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Table 1 γ -Ray Counts Observed for SGR XR-2

Photon energy	Counts observed from source region	Counts expected from background	Excess No. of counts
200 MeV	48	34.1	13.9
500 MeV	17	10.0	7.0
1 GeV	8	2.5	5.5
2 GeV	2	1.0	1.0

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Observations of D-Region Modifications at Low and Very Low Frequencies

WE describe here an experiment in which the modulation radiated by a high frequency radio transmitter is transferred by an ionospheric process to waves of low and very low frequency (LF and VLF). This is the first observation of so called "cross modulation" for waves of frequencies below 167 kHz and this result has important implications regarding the electron energy and collision frequency in the lowest regions of the ionosphere.

Recent experiments¹ have shown that considerable modification of the ionospheric F-region can result through heating produced by high power, high frequency transmissions reflected from this layer. Because an appreciable amount of the radiated power is absorbed in the lowest region of the ionosphere (D-region) we attempted to detect modification effects at these heights. Conventional ionosonde techniques are insensitive to changes in the D-region, consequently a method utilizing VLF and LF waves was developed because waves of these frequencies are reflected exclusively from D-region heights.

The frequency of the modifying transmitter can be varied between 5 and 10 MHz and the maximum effective radiated power is estimated to be equivalent to 50 MW into an isotropic antenna. For these frequencies the maximum D-region (non-deviative) absorption occurs at roughly 85 km; at this height 5 MHz disturbing radiation will increase the electron temperature by roughly 30% for a radiated power of 20 MW. Assuming that the electron collision frequency ν is linearly related to the electron temperature² we expect an increase in ν of 30%. The change in ν will, of course, be smaller above and below the level of maximum absorption.

The change in electron collision frequency will affect the propagation characteristics of VLF and LF waves reflected from the disturbed region. The 20 and 60 kHz transmissions, from stations WWVL and WWVB, were therefore monitored at a location chosen so that the reflexion points were directly above the disturbing transmitter (Fig. 1). To eliminate the ground wave, the signals were received on a loop antenna set at right angles to the plane of propagation. The disturbing transmitter was 100% amplitude modulated at 10 Hz; any changes induced in the VLF and LF signals as a result of ionospheric modification would occur at this frequency, because the relaxation time of the D-region is less than a few ms. Further, in order to identify unambiguously the interaction of the disturbing transmissions with the VLF and LF signals (the cross modulation), the transmitted 10 Hz signal was varied in phase at the rate of 1 cycle per 6 min. Thus a sinusoidal output with a 6 min period was obtained from the

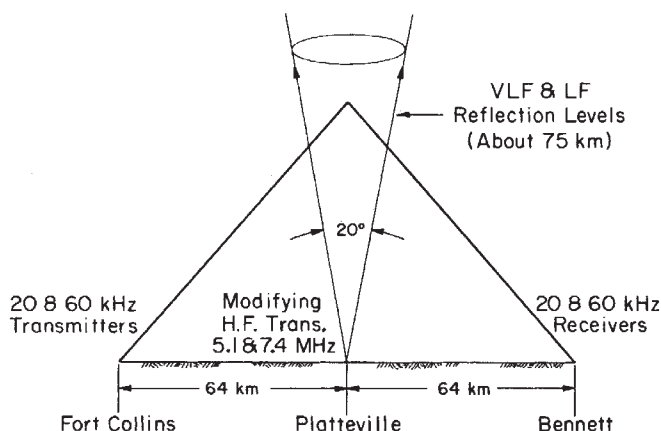


Fig. 1 Location of the VLF and LF transmitting and receiving sites relative to the modifying transmitter at Platteville.

coherent 10 Hz detector in the receiver when the transmitted modulation was present on the VLF and LF waves. The maxima and minima in the detector output correspond to the in-phase and anti-phase positions of the transmitted and reference 10 Hz. Typical records indicating the presence of 10 Hz cross modulation on both 20 and 60 kHz signals are shown in Fig. 2. The lowest frequency on which such an effect has previously been reported is 167 kHz (ref. 3).

The cross modulation was principally an amplitude effect and only small changes were noted in the signal phase. The changes recorded at 60 kHz were greater than those at 20 kHz. Cross modulation was observed for disturbing wave frequencies of 5.1 and 7.4 MHz, although the effects were smaller for 7.4 MHz because of the decrease in D-region absorption with increasing wave frequency. Typical values of the disturbances observed are presented in Table 1.

We have attempted to account for the observed results in terms of the electron density and collision frequency distributions in the D-region.

Full wave calculations of the phase and amplitude of 20 and 60 kHz signals propagated over the experimental transmission path indicate that these parameters are particularly sensitive to the assumed distribution of electron density with height. Modelling of the collision frequency change necessary to produce the observed results is thus restricted by the validity of the electron density profile adopted for the calculations. In the absence of the disturbing transmissions the phase and amplitude observations were adequately represented by the winter noon model of electron density and the collision frequency profile proposed by Deeks⁴.

For the models tested, the calculations predict much larger phase and amplitude effects than those observed. The predicted phase shifts and amplitude changes for 60 kHz are greater than those for 20 kHz. Assuming an effective reflexion height of 70 km, the heated section of the D-region has a diameter of roughly 25 km, and the mean diameter of the first Fresnel zone is roughly 63 km for the 20 kHz transmission and roughly 36 km for the 60 kHz transmission. The heated area therefore represents only approximately 1/6 of the first

Table 1 Cross Modulation Effects in the Abnormal Loop Signal*

Modifying frequency (MHz)	Transmission frequency (kHz)	Amplitude change (%)	Phase (degrees)
7.4	20	0.19	0.23
7.4	60	0.35	0.03
5.1	20	0.29	0.3
5.1	60	0.53	0.07

* 7.4 MHz experiment carried out from 1100 to 1200, 105° Western Meridian Time; 5.1 MHz experiment carried out from 1300 to 1400, 105° WMT.

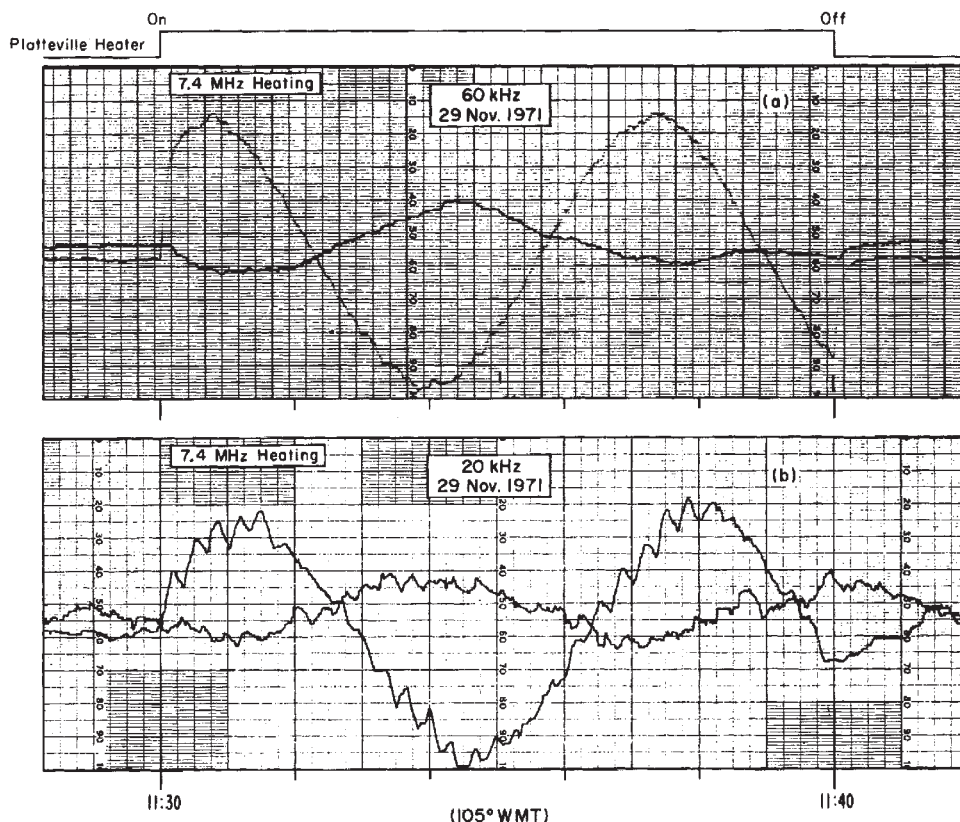


Fig. 2 Cross modulation effects recorded on the 20 and 60 kHz transmissions for a disturbing frequency of 7.4 MHz.

Fresnel zone at 20 kHz and approximately 1/2 of the first Fresnel zone at 60 kHz. The discrepancy between the calculated and the observed changes may arise because the signals reflected from the modified part of the ionosphere represent only a fractional part of the signal recorded. Further, the stronger effect at 60 kHz compared with that at 20 kHz may result from the three-fold increase in the proportion of the Fresnel zone that is involved in the reflexion process, as well as from the fact that the 60 kHz transmission is reflected from close to the level of maximum absorption.

The presence of cross modulation effects on VLF and LF waves confirms that the very lowest regions of the ionosphere are modified by the high power high frequency transmitter at Platteville, Colorado. The magnitude of the cross modulation depends on the frequencies of both the wanted and disturbing waves. We also expect the effect to be dependent on the electron density height profile, the power of the modifying transmissions, and the propagation path of the diagnostic wave.

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Late Cambrian Fossils and the Age of the Ross Orogeny, Antarctica

ONE of the unsolved geological problems of East Antarctica has been the age of the folded sediments and metasediments unconformably underlying the near horizontal late Palaeozoic and early Mesozoic Beacon Supergroup of Victoria Land (Fig. 1). The known occurrence of trace fossils, including *Ruzophycus*, indicates a Phanerozoic and probably Cambrian age for at least the uppermost part of the folded sequence¹. The discovery of late Cambrian body fossils in the upper part of this sequence establishes, for the first time, a definite minimum age for the succession and enables correlation with early Phanerozoic sequences elsewhere in Antarctica.

The fossils occur in the Bowers Group, a largely clastic

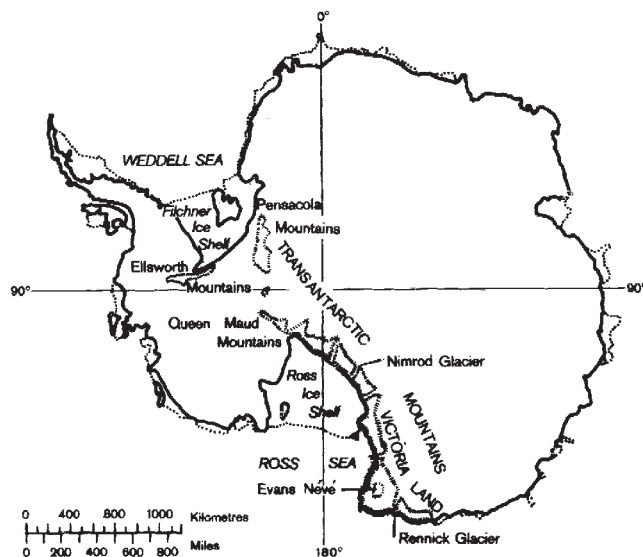


Fig. 1 Locality map of Antarctica.

sequence which attains an inferred maximum thickness of 25 km (ref. 1). In its area of greatest development the group has been divided into three formations. The oldest, the Carrier Conglomerate, consists chiefly of polymict conglomerate; the overlying Sledgers Formation consists chiefly of sandstone, mudstone, and basic volcanic flows; and the youngest division, the Camp Ridge Quartzite, consists of orthoquartzite and quartz conglomerate.

The fossils occur in the southernmost known outcrops of the Bowers Group, where it flanks the eastern side of the Evans N  v   (a high plateau from which glaciers drain to the east and south into the Ross Sea and to the west and north into the Antarctic Ocean). To the south the succession is either faulted out or removed by erosion, and only granite or older rocks belonging to the Robertson Bay Group² crop out. In the Evans N  v   area approximately 1,600 m of shallow marine Bowers Group sediments is exposed. The sequence consists of fissile mudstone in the lower half, grading upwards into sandstone, with localized limestone lenses in mudstone and sandstone (Fig. 2). The uppermost 360 m of the marine sequence consists chiefly of red sandstone with rare fossils, arthropod tracks and mudcracks, suggesting very shallow marine conditions, probably in part intertidal. A limited thickness of the probably fluvial quartzose sandstone and conglomerate of the Camp Ridge Quartzite rests on the red beds, abruptly but apparently conformably. The shallow marine sequence immediately below the Camp Ridge Quartzite is probably the time correlative of part of the Sledgers Formation. No correlative of the Carrier Conglomerate is exposed.

The fossils, chiefly brachiopods and trilobites, are most prolific in the lowermost mudstone unit, but scattered horizons rich in brachiopods also occur in slightly calcareous sandstone overlying the mudstone unit. Oolitic limestone, which forms lenses in mudstone higher in the sequence, locally contains fragments of hyolithids, of the *Orthotheca* type. Rare, inarticulate brachiopods were found associated with the red beds higher in the succession. No fossils or fossil traces were noted in the overlying Camp Ridge Quartzite.

The brachiopods (examined by D. I. MacKinnon, University of Canterbury, Christchurch) are represented by an unnamed species of *Billingsella*, and by an inarticulate form, considered to be an obolid. The genus *Billingsella* has a worldwide distribution, occurring in middle to late Cambrian rocks in North America and Europe³, Eastern Asia⁴, Australia⁵ and Antarctica⁶. R. A. Cooper (New Zealand Geological Survey) has noted (personal communication) that "the trilobites include *Pseudagnostus* Jackel, a dikelocephalean, and at least four other incompletely preserved polymerid genera. Hyolithids include *Orthotheca*. Gastropods, both orthostrophic and hyperstrophic (macluritean) forms, are also present. *Pseudagnostus* is a typically upper Cambrian form, only one species being known from the middle Cambrian and one from the Tremadocian⁷." We believe therefore that the fauna is late Cambrian.

The general composition of the fauna resembles that of the Heritage Group in the Ellsworth Mountains of West Antarctica⁶ (Fig. 1). The new Victoria Land discovery, however, includes the youngest fossils so far recorded from the basement rocks of East Antarctica. Older Cambrian faunas are known from elsewhere in the Transantarctic Mountains, of which the Evans N  v   area forms a part. Previous fossil discoveries made from the Pensacola Mountains (at the Weddell Sea end) and the Queen Maud Range include trilobites of middle Cambrian age⁸, and archaeocyathids from the Nimrod Glacier area indicate a late early-Cambrian age⁹.

The new fossil discovery provides evidence that a Cambrian geosyncline or series of geosynclines existed along the whole of the present extent of the Transantarctic Mountains, 3,000 km. It also provides palaeontologic evidence for the maximum age of the Ross Orogeny¹⁰, which folded the lower Palaeozoic and late Precambrian strata. The orogeny was accompanied and

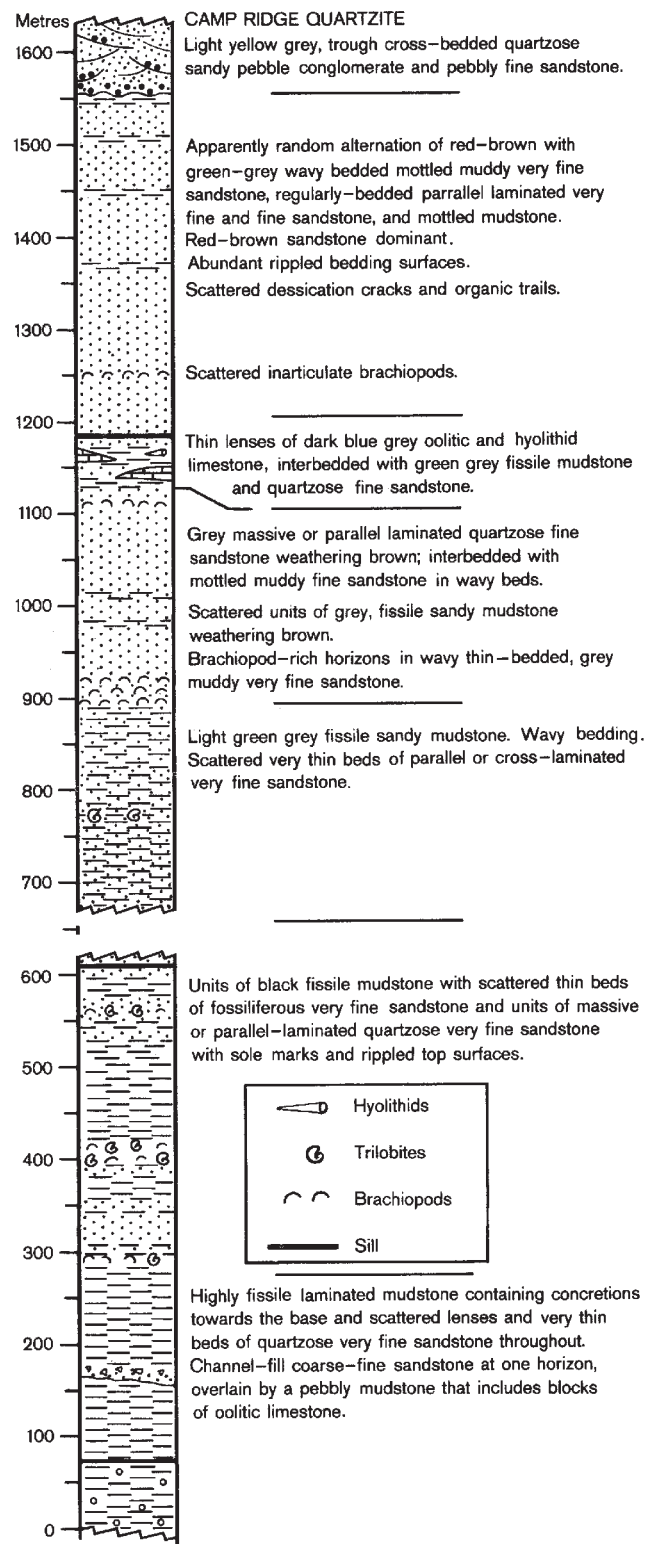


Fig. 2 Composite stratigraphic column, Bowers Group, Evans N  v  .

followed by widespread intrusion of granite belonging to the Granite Harbour Intrusive Group¹⁰. In Victoria Land radiometric dating of these granites gives ages between 400 and 530 m.y.¹¹⁻¹³. The maximum ages agree well with the palaeontological data, and it seems that the initiation of the Ross Orogeny must have coincided with or closely followed the deposition of the fossiliferous sediments in the Evans N  v   area. The regressive nature of the sequence suggests that precursor movements to the Ross Orogeny may already have begun. The often coarse, largely terrestrial deposits forming

the Camp Ridge Quartzite indicate emergence and the development of topographic relief, and they may represent cessation of marine sedimentation and emergence of the whole area.

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The Direct Viewing and Brief-time Recording of Crystallographic Events

THE exposure times involved in obtaining X-ray diffraction patterns are an obstacle to the study of dynamic processes. Several workers have tried to devise visual presentations of the diffraction patterns from transient crystallographic events, and recently Chester and Koch¹ reported some success using a vidicon tube which was sensitive to X-rays and which had a complex diode mosaic as photocathode. Other workers, including ourselves, have used a phosphor as an image converter, the visual image produced by the phosphor being amplified by a high-gain image intensifier. We have had some success with an image intensifier that has a television read-out, but the equipment is expensive, cumbersome, and has to be custom-built. Here we report that acceptable results can be obtained using relatively cheap standard night surveillance equipment currently being produced in the USA and available in the UK. These units are designed to be carried in the hand or even mounted on a rifle and are extremely compact and portable.

The 'Owl Eye' intensifier, for example, was developed by the Aerojet Delft Corporation of California for police work². It gives a 125 mm diameter visual read-out which may readily be viewed by eye from a working distance of several feet and can be photographed with a standard camera. The result illustrated in Fig. 1 was obtained with this unit. X-rays from an 'Elliott GX6' generator operating at 40 kV and 50 mA were emitted through a 1.5 mm diameter exit port to give a slightly divergent beam. The specimen consisted of a thin copper grid used as the specimen support in electron diffraction work. This was positioned directly over the exit port, the main beam being trapped by a lead stop set in a brass saddle and the diffraction pattern being transmitted via lateral cutaways.

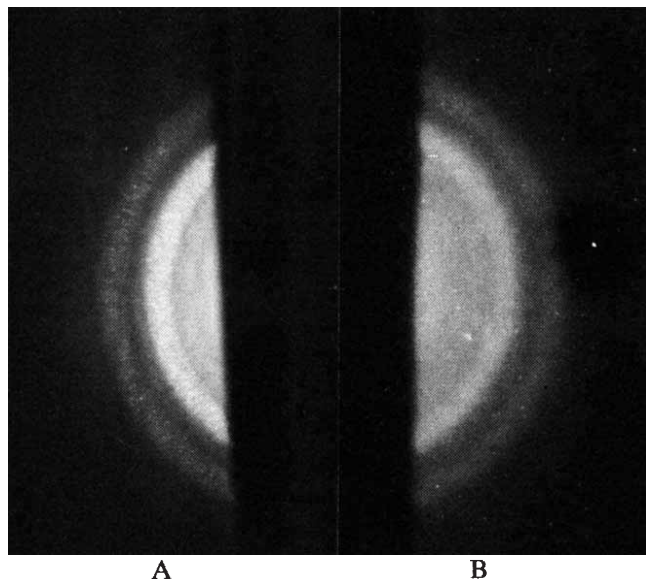


Fig. 1 Copper K interference pattern obtained from a polycrystalline copper specimen. *A* was recorded directly on fast X-ray film and required an exposure of 30 s. *B* was obtained by photographing a visual image on the image intensifier and required an exposure of 1/30 s.

To use the 'Owl Eye' image intensifier the X-ray diffraction pattern was first converted into a low light level visual image. This was done with a transmission fluorescent screen obtained from the trade and consisting of a silver activated zinc/cadmium sulphide phosphor (grade YSF) of particle size 25–30 μm . This is deposited with a suitable binder on to a thin melinex film to a density of about 35 mg per cm^2 . The 'Owl Eye' system was then focused on this screen, viewing for reasons of radiation safety in a mirror set at 45°. Preliminary focusing was done with the X-ray set switched off and the room dimmed.

With the laboratory blacked out a bright image of the copper K_α reflexions from the (111) and (200) crystal planes of the copper specimen was readily obtained, the copper K_β reflexion from the (111) plane being just discernible. Using HP4 film developed in Microphen developer, and a 35 mm camera set at f2.0, a well balanced photographic negative was obtained in 1/30 s. A print of such an exposure is shown in Fig. 1 compared with a similar print taken from a direct X-ray exposure made on fast dental film in 30 s. Loss of detail is obvious, but this detracts only slightly from the value of the result as an effective brief time record.

With exposures of 1/30 s, the frame-by-frame recording by cine-camera of a sequence of short-lived crystallographic events becomes possible.

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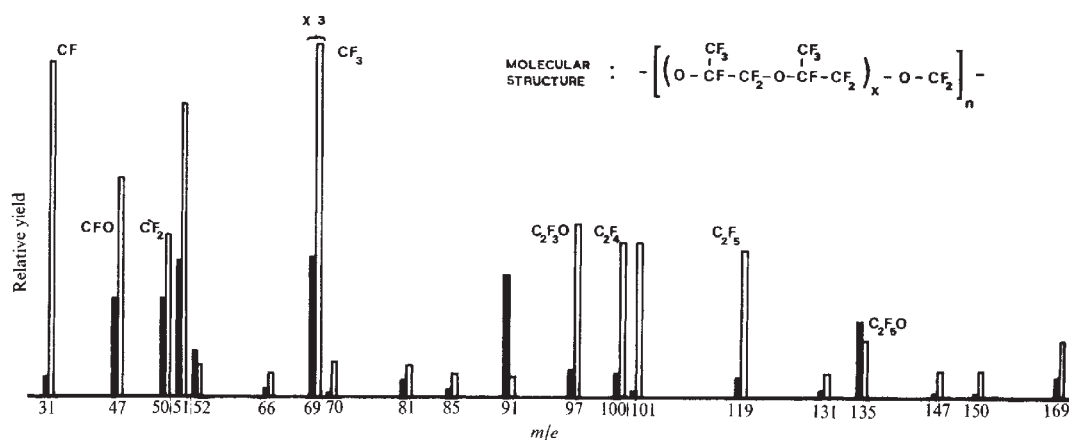
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Perfluoropolyether—a Vacuum Pump Fluid resistant to Electron Induced Polymerization

GROWTH of polymer films on the components of electron beam instruments is a problem experienced widely because of the extensive use of oil pumps on such apparatus. Recently, new thermally stable fluids have become available which are based

Fig. 1 Mass spectra of: black columns, atmosphere above the vapour stream pump at a chamber pressure of 2.5×10^{-6} torr; white columns, 'Fomblin' components admitted from source at 190° C and with chamber pressure of 2×10^{-6} torr.



on oxygen linkage of fluorocarbon groups; we have found that they can be used in vacuum pumps without the attendant polymerization problems. An earlier study¹ has shown that a high molecular weight ($\sim 6,500$ a.m.u.) perfluoropolyether fluid has lubricating properties suitable for use in rotary pumps. The work indicated that a similar fluid of greater volatility (molecular weight $\sim 3,000$ a.m.u.) should be suitable for use in vapour stream pumps and here we give conclusive evidence of its value for this purpose. The fluids, known as 'Fomblin', are prepared for us by Montecatini Edison S.p.A. (see ref. 2).

The fluid to be tested was used in a vapour pump, Edwards EO4 5 inch diameter aperture, connected to a stainless steel chamber of 28 l. capacity. A rotary pump with a nominal speed of 35 l./min and charged with 'Fomblin' backed the vapour pump. The chevron baffle mounted in the vapour pump inlet was uncooled to ensure a high backstreaming level. The pressure measured by an ion gauge was 10^{-6} torr and the fluid was in use for a total of 1,500 h. We fitted an Edwards mass spectrometer (3 inch radius 60° magnetic sector field) with its unprotected (nude) ion source in the chamber, so that fragmentation spectra could be recorded for the vacuum atmosphere and vapour from the parent fluid injected close to the ion source.

Fig. 1 shows comparative spectra from a sample of unused 'Fomblin' evaporated from a side oven at 190° C and the backstreaming vapour from the vapour pump after 600 h use. Possible compositions of the molecular fragments are also given in Fig. 1. Mass spectra were generally measured up to an m/e ratio of 260, but the yield of fragments above 170 was small and we have not included them. Although the relative abundancies differ, the similarity between the molecular fragments in the two spectra suggests to us that they both arise chiefly by electron impact within the ion source of the mass spectrometer.

Further extensive tests were made by bombarding metal and glass slides with electrons for 1 h, first, in the presence only of backstreaming vapour from the pump, and second, with the fluid spread on the target. The electrons had an energy of 1 keV and a current of 0.5 mA was recorded with a target area of 25 cm^2 . Occasionally, we could detect signs of polymer formation on the target but this was not unexpected because the gas analysis showed traces of hydrocarbons in the system from previous use of the pump with conventional fluids. But, as these impurities disappeared, so did the formation of polymers. The mass spectrometer study showed that electron impact will fragment the perfluoropolyether molecule, so we conclude that neither the parent molecule nor its fragments can be polymerized by electron bombardment either in the liquid or the adsorbed state.

We note that if a mass spectrometer with a nude source, as used in these experiments, had been operated at room temperature in the saturated vapour of any of the hydrocarbon or silicone pump fluids, then serious contamination and failure of the ion source would have ensued within a few hours.

The nude ion source of the instrument was operated in the saturated vapour of the fluid for 50 h and for a further 100 h in backstreaming components from the pump without deterioration in its performance.

Because the fluid can be used in rotary mechanical and vapour stream pumps it could prove valuable for pumping systems on electron microscopes and ion beam accelerators where secondary electrons abound. It will be necessary to ensure that molecular fragments of the fluid are not reactive with specimens or vacuum materials.

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Electrochemical Photolysis of Water at a Semiconductor Electrode

ALTHOUGH the possibility of water photolysis has been investigated by many workers, a useful method has only now been developed. Because water is transparent to visible light it cannot be decomposed directly, but only by radiation with wavelengths shorter than 190 nm (ref. 1).

For electrochemical decomposition of water, a potential difference of more than 1.23 V is necessary between one electrode, at which the anodic processes occur, and the other, where cathodic reactions take place. This potential difference is equivalent to the energy of radiation with a wavelength of approximately 1,000 nm. Therefore, if the energy of light is used effectively in an electrochemical system, it should be possible to decompose water with visible light. Here we describe a novel type of photo-electrochemical cell which decomposes water in this way.

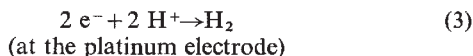
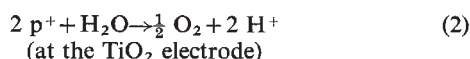
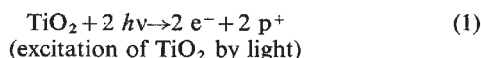
Electrolysis of water can occur even without applying electric power if one of the following three conditions is fulfilled. First, oxygen evolution occurs at a potential more negative than that at which hydrogen evolution occurs in normal conditions; second, hydrogen evolution occurs at a potential more positive than that at which oxygen evolution occurs in normal conditions; third, the potential for oxygen evolution is made more negative and that for hydrogen evolution is made more positive, until the former is more negative than the latter.

Current-voltage curves of a semiconducting n-type TiO_2

electrode have been measured with a static potentiometer in the dark and under irradiation with light² (Fig. 1). Anodic current which is proportional to the intensity of light begins to flow for wavelengths shorter than 415 nm, that is 3.0 eV, which corresponds to the band gap of TiO₂. The current reaches saturation at potentials positive relative to a saturated calomel electrode (SCE). These facts suggest that the anodic reaction is related to the formation of holes in the valence band by light excitation. Oxygen evolution was confirmed by several means of analytical measurements^{3,4}. Oxygen evolution occurs at -0.5 V (SCE) in an aqueous electrolyte of pH 4.7; this is more negative than the standard potential. We have termed such behaviour "photosensitized electrolytic oxidation" (ref. 2). When halogen ions were introduced in the electrolyte, they were also oxidized through the suggested mechanism of photosensitized electrolytic oxidation. This also occurred with other types of n-type semiconductor such as ZnO and CdS (ref. 5). We believe therefore that the oxygen evolution reaction on the TiO₂ electrode under irradiation belongs to the first category described above.

We then constructed an electrochemical cell in which a TiO₂ electrode was connected with a platinum black electrode through an external load (Fig. 2). When the surface of the TiO₂ electrode was irradiated, current flowed from the platinum electrode to the TiO₂ electrode through the external circuit. The direction of the current reveals that the oxidation reaction (oxygen evolution) occurs at the TiO₂ electrode and reduction (hydrogen evolution) at the platinum black electrode.

We suggest that water can be decomposed by visible light into oxygen and hydrogen, without the application of any external voltage, according to the following schemes:



The overall reaction is



The starting potential of the oxidation reaction at the TiO₂ electrode corresponds almost exactly to the flatband potential which is constant in the electrolyte solution of a given pH. To

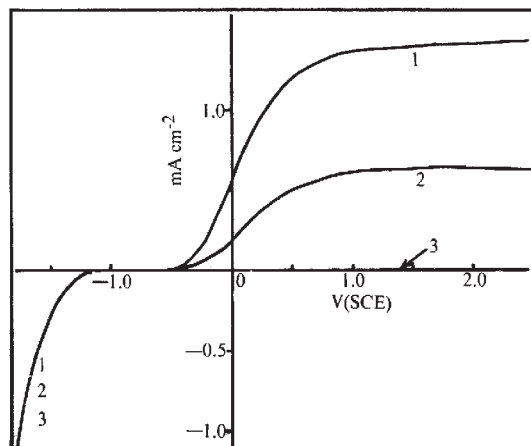


Fig. 1 Current-voltage curves for TiO₂ n-type semiconductor. A single crystal wafer of n-type TiO₂ (rutile) was used after treatment at 700° C at 10⁻⁴ ~ 10⁻⁵ torr for roughly 4 h to increase the conductivity of the crystal. This wafer was approximately 1.5 mm thick and the exposed (001) surface area was approximately 1.0 cm². Indium was evaporated on to one side of the surface to ensure ohmic contact and a copper lead wire was connected on the indium layer with silver paste. All other surfaces were sealed by epoxy resin.

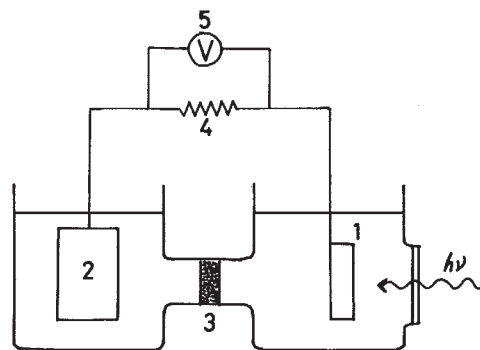


Fig. 2 Electrochemical cell in which the TiO₂ electrode is connected with a platinum electrode (see text). The surface area of the platinum black electrode used was approximately 30 cm².

increase the efficiency of the decomposition process, more reducible species, for example, dissolved oxygen or Fe³⁺ ions, must be added in the compartment of the platinum electrode. When Fe³⁺ ions were added, the current produced under irradiation increased. Currents of a few mA flowed when the TiO₂ electrode (surface area ~1 cm²) was irradiated by a 500 W xenon lamp; we estimate the quantum efficiency in this case to be approximately 0.1. The e.m.f. of the cell was measured to be up to 0.5 V.

It is possible that the hydrogen evolution reaction shifts towards more positive potential than normal when suitable p-type semiconductor electrodes are irradiated, in the same way that photosensitized oxygen evolution occurs with n-type semiconductor electrodes. If such a p-type semiconductor electrode is used instead of the platinum electrode, electrochemical photolysis of water may occur more effectively.

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BIOLOGICAL SCIENCES

One and Two-dimensional Structure of Alpha-Helix and Beta-Sheet Forms of Poly(L-Alanine) shown by Specific Heat Measurements at Low Temperatures (1.5–20 K)

HOMOPOLYPEPTIDES provide good model systems for various aspects of proteins^{1,2}. Recent advances in high polymer and solid state physics have enabled the vibrational aspects of the simpler homopolypeptides to be treated as normal—but complicated—polymers by the theoretical techniques of lattice dynamics based on the experimental methods of neutron, infrared and Raman spectroscopy. Basically, however, these latter methods examine the optical vibrational modes of a system, that is, those modes which are of energy higher than, for example, 70 cm⁻¹. The important lower-energy modes are

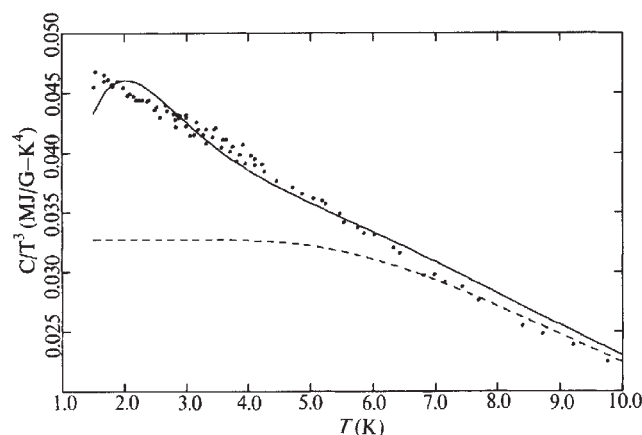


Fig. 1 Poly(L-alanine) in alpha-helix form: specific heat divided by cube of temperature versus temperature: ●, experimental points; —, fitted to a Tarasov one and three-dimensional model, with an independent set of vibrators (Einstein mode). Compare ---, a fit to the one and three-dimensional model only.

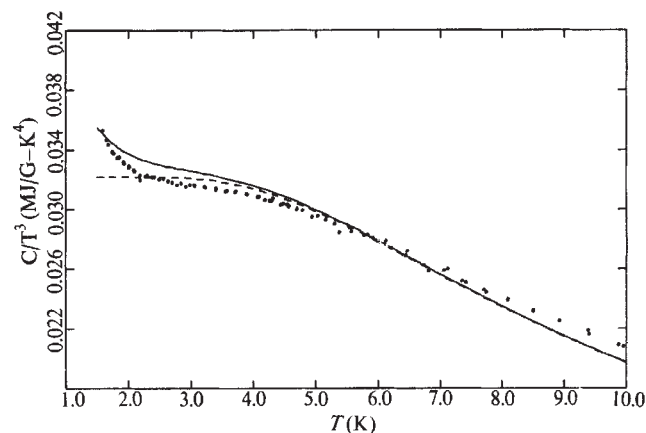


Fig. 2 Poly(L-alanine) in beta-sheet form: specific heat divided by cube of temperature versus temperature: ●, experimental points; —, fitted to a Tarasov one and two-dimensional model, with an independent set of vibrators (Einstein mode). Compare ---, a fit to the two and three-dimensional model only.

well examined by specific heat measurements, which do not require single crystal samples, at low temperatures (for example, 1–20 K, where only the acoustic modes are excited thermally to any appreciable extent—1 K $\sim$ 0.7 cm^{-1}). The low temperature measurements presented here are the first made on a polypeptide in alpha and beta-conformation, and clearly show the one-dimensional nature of the α -helix and the two-dimensional nature of the β -sheet structure. Quantitatively, these measurements also provide tests of theories of published vibrational models of homopolypeptides, and will give hydrogen bond strengths.

At sufficiently low temperatures, the specific heat, C , of any one-dimensional non-metallic solid is proportional³ to temperature T^1 (measured in K from absolute zero), for a two-dimensional solid $C \propto T^2$, and for a three-dimensional solid $C \propto T^3$. The constants of proportionality depend on the interatomic forces, and are usually expressed as Debye "effective temperatures" θ_1 , θ_2 , θ_3 respectively. For simplicity, we use the Tarasov model^{4,5}.

Our poly(L-alanine) (PLA) was polymerized by the N-carboxyanhydride method⁶ by Dr Masanao Oya into predominantly α and β forms⁷.

Details of the sample characterization and of the specific heat apparatus and methods are given elsewhere^{8,9}.

The results above 10 K are easily fitted to the Tarasov two-parameter models, showing definite one and three-dimensional (α) or two and three-dimensional (β) molecular contributions to the specific heat. (The gram-molecular weight of the vibrating unit was taken as 71.08.) Figs. 1 and 2 show the results below 10 K, conveniently scaled. For both forms the experimental data rise markedly above the Tarasov fits extended to lower temperatures (dashed lines) (α : $\theta_1 = 321$ K, $\theta_3 = 51$ K; β : $\theta_2 = 156$ K, $\theta_3 = 34.9$ K), giving evidence for another contribution to the specific heat. Specific heat bumps have been observed in other polymers and glasses^{8,9}. We, too, assume empirically that the solid contains a small mole-fraction f of Einstein oscillators³ with characteristic temperature θ_E . The corresponding fits (that is, α , one and three-dimensional Tarasov model, plus a set of independent oscillators; β , one and two-dimensional Tarasov model, plus a set of independent oscillators) are quite good (standard deviations: α 4.2%, β 3.1%) with parameters for the α -form $\theta_1 = 321$ K (1.9%), $\theta_3 = 51$ K (3.6%), $\theta_E = 9.95$ K (0.5%), $f = 0.00175$ (0.1%), and for the β -form $\theta_2 = 156$ K (6.6%), $\theta_3 = 34.9$ K (0.3%), $\theta_E = 2.5$ K (0.01%) and $f = 0.000040$ (0.05%). The numbers ($x\%$) are the change in the s.d. of the total specific heat fit when the parameter value is increased by 5% from the values quoted. Note that the fit is relatively insensitive to the value of f .

The origin of the Einstein modes is not clear^{8,9}. It has been suggested¹⁰ that a tunnelling state due to the rotation of an (impurity) hydroxyl group would have an energy splitting of about 10^{-4} eV, and so could be an explanation for the specific heat bump observed in PLA.

A priori calculations, via lattice dynamics, have been made on PLA in the α ^{11–13} and β ¹⁴ conformations, but the published data have not yet been presented with sufficient resolution for comparison with our results. Thus, we hope that theorists will be encouraged to make the necessary relatively small extensions to their elaborate computer calculations.

These measurements will be valuable in the basic thermodynamics of polypeptides¹⁵, and in theories of enzyme action, where the low-frequency modes of vibration may well be important. This method might also be a feasible way of estimating the proportion of α to β structure present in an enzyme.

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Note added in proof. Reasonable agreement has been found between our α -PLA data and those calculated from the frequency distribution of ref. 11 (B. Fanconi, private communication). The specific heat of α -PLA has been measured (1.3–4.3 K) also by P. Delhaes, M. Daurel and E. Dupart (*CR Acad. Sci.*, **274**, 308; 1972).

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Performance of Composite Foetal Hearts

WE wished to study the interactions between pieces of foetal heart tissue of different origins, to see if they could influence the healing of myocardial infarctions, or the pacing of the ventricles in heart block. We had established a baseline for the performance of intact foetal hearts and slabs of foetal heart tissue in culture, and related it to the stage of development in rat, mouse, cat, rabbit and man, showing that less developed foetal hearts live longer, and beat better than older foetal hearts¹. We extended this technique to examine the effects of parts of foetal hearts on each other, because it gives consistent results, uncomplicated by rejection, other than that originating in the cells concerned. We used hearts of litter mates, hearts from different species, at different stages of development, and across the species barrier. As controls we used hearts sectioned and re-assembled.

Hearts were cut vertically into two along the atrial and ventricular septa and cultured with the cut surfaces touching in as nearly normal position as possible. They were put on stainless steel wire mesh—bridge-shaped grids—with a mesh size of 0.1 mm, and were arranged so that the lower surfaces of the hearts just touched the culture medium (Burrroughs Wellcome 199 plus 35% colostrum-deprived calf serum, plus 50 µg/ml. insulin, plus 0.1 µg/ml. cortisol). The cultures were incubated at 37° C, in 95% oxygen and 5% carbon dioxide, as described before¹.

Four series of experiments were done and each examined one set of variables. The beating rates of the two halves were recorded after 12, 19 and 24 h and thereafter every 24 h. The time taken for the beating of the half hearts to synchronize was noted, and then a sample of joined hearts from each series was placed in 10% formal saline and examined histologically. The hearts were serially sectioned at 7 µm, and stained with Ehrlich's haematoxylin and eosin, or Verhoeff's elastic stain and Van Gieson stain.

The control hearts (group 1) consisted of mouse and rat hearts sectioned and rejoined at various stages of development.

Mouse hearts were three 12-day (55% term), ten 13-day (59% term), three 15-day (68% term), three 16-day (73% term), and thirteen 17.5-day (80% term) hearts. Two 16-day foetal rat hearts (68% term) were included together with the 15-day mouse hearts, to serve as a control for group 3 (described later).

Mouse and rat hearts from different foetuses in the same litter, sectioned and cultured in apposition, at various stages of development constituted group 2. The mouse litter mate hearts were three 12-day (55% term), four 15-day (68% term), six 16-day (73% term) and seventeen 19.5-day (88% term). The rat litter mate hearts were four 16-day (68% term).

Six hearts from different species at the same stage of development, cultured in apposition, constituted group 3. These were 15-day (68% term) mouse hearts and 16-day (also 68% term) rat hearts.

Group 4 consisted of five hearts at different stages of development from cat foetuses at 21 days (33% term) and mouse foetuses at 18 days (82% term).

All composite hearts except the mouse/cat chimeras, which became contaminated, were beating synchronously after 144 h. Less developed hearts synchronized earlier than more fully developed foetal hearts. In all cultures, the beating rate assumed by the composite organ was the rate of the faster half-heart.

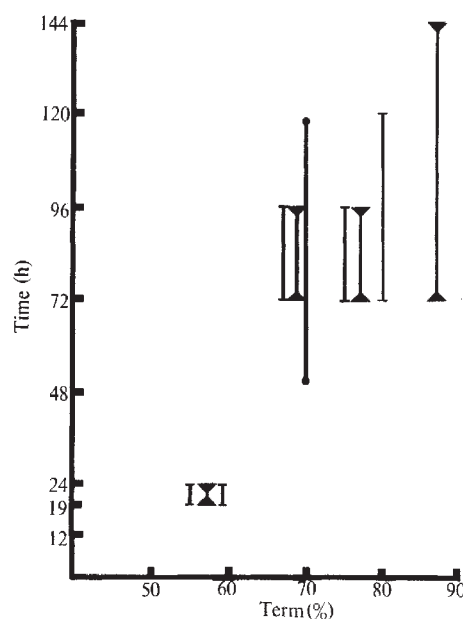


Fig. 1 Relation of heart maturity to synchronized beating. Observed time of onset of synchronized beating and the time when all the hearts in the series were beating synchronously related to the percentage of full term development. Since we only removed the hearts from the incubator at set intervals to observe the beating, the actual time of onset is not recorded. —○—, Rejoined heart; —●—, composite heart, same species; —●—, composite heart, different species.

There was no significant difference in performance between composite hearts made from halves of the same or different species, or noticeable difference from the survival and performance of intact hearts at comparable stages of development.

The record of beating performance is of necessity incomplete, because it was only possible to examine the cultures by removing them from their optimal environment in the incubator, with consequent damage. Thus our results relate to discrete observations at 12, 19, 24, 48, 72, 96, 120 and 144 h. We used alternate observations as an opportunity to change the culture medium.

All hearts were firmly adherent at the time of fixing. They were mostly taken for section as soon as the whole batch was beating synchronously. Some were left for comparison with intact hearts. The young hearts achieved synchronization sooner than the more mature hearts. They also had less collagen in the junctional area, even when cultured for longer periods, comparable with the time taken for the more mature hearts to achieve synchrony. In all composite hearts, collagen never forms an unbroken barrier across the join. Heart muscle is continuous with the muscle on the other side. No collagen was deposited in the junctional area in hearts whose components were younger than 60% of term. The cells in the junctional areas tended to align themselves to lie along the junctions, so that the line of union was obvious. We could see no evidence of tissue conflict in the junctional areas, but in the more mature hearts the presence of collagen suggests the formation of scar tissue rather than muscle fusion.

When the fusion of foetal gonad and lung tissues from widely different species was demonstrated⁴⁻⁶, the connecting and epithelial tissues migrated and fused intimately, and we hoped for a similar result. Certainly with the young foetal hearts the muscle cells joined and there was no evidence of collagen formation; with the older hearts, however, some collagen was formed. One possible reason for this is mechanical; the older hearts are firmer than the younger hearts. When they are cut in half, they do not flatten and spread out as do the younger hearts and each beat moves the whole mass of muscle more violently, and prevents the cut surfaces staying in close contact. Another reason may be that the cells in the older hearts are

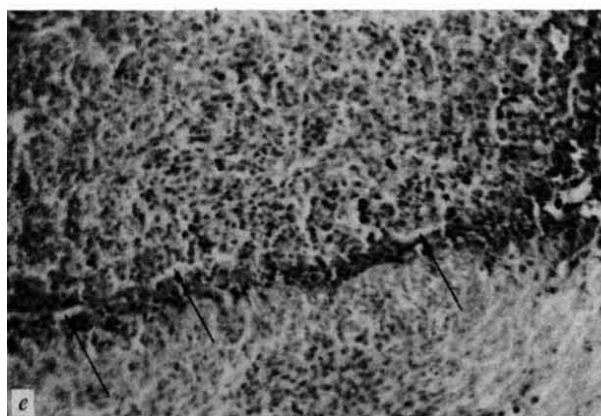
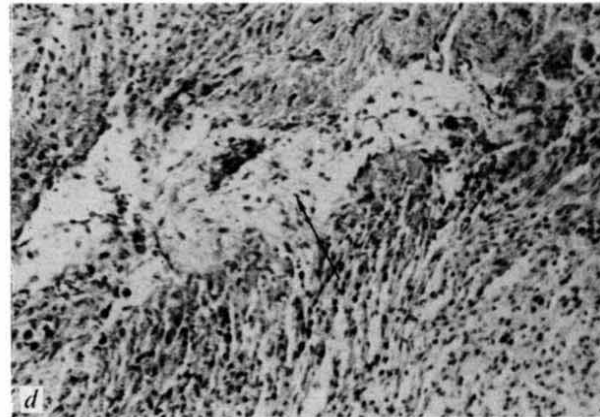
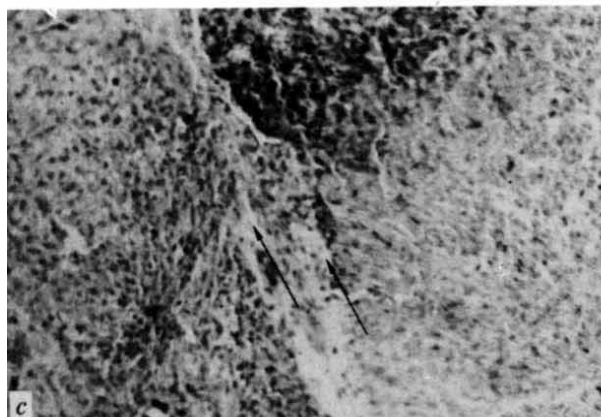
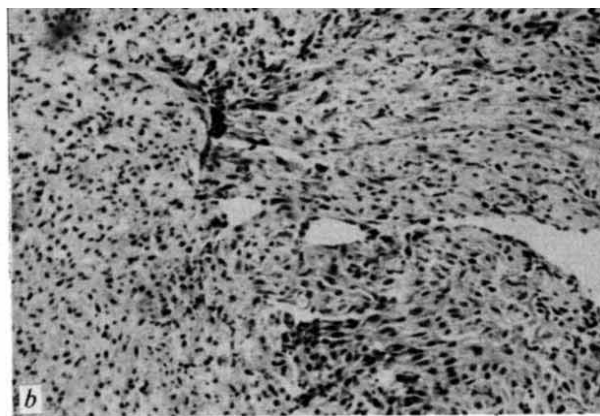
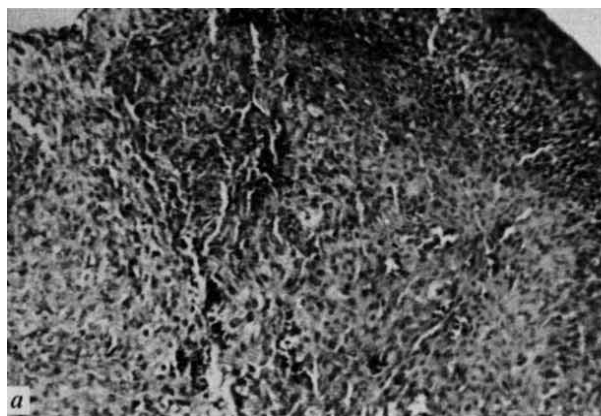


Fig. 2 The photomicrographs show sections through the line of fusion of various combinations of foetal heart tissue. The collagen deposited in the join is arrowed. *a*, Section through the join of litter mate hearts (55% term). The myocardial cells are in surface contact and some are aligned along the axis of the join. *b*, Section through the join of two halves from the same heart (68% term) showing the intimate contact between the myocardial cells. *c*, Section through the join of litter mate hearts (68% term). The myocardial cells are in contact but there is some collagen deposition separating the muscle bridges. *d*, Section through the join of litter mate hearts (80% term). There are bridges of myocardial cells crossing the collagen, which has been deposited in greater amounts than in the younger hearts. *e*, Section through the join of half hearts from different species (rat and mouse, both 68% term). The muscle cells from both species are in contact and there is only a small amount of collagen.

more differentiated and not so adaptable, allowing fibroblastic invasion or metaplasia.

The younger and older foetal hearts not only adhered, but there was intimate adhesion of the muscle cells with sufficient continuity for synchronization of the beating rate of both halves. Furthermore, there is histological evidence of bridges of cells in the junctional tissue, both within the species and across the species barrier.

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Hypothesis for a Neuronal Mechanism involved in Memory

It is generally agreed that learning and memory involve a long-lasting alteration in synaptic transmission¹. The efficiency of synaptic transmission depends on the amount of transmitter released from the presynaptic terminal and on the response of the postsynaptic element. The present model combines recent results from a number of independent lines of research to provide a mechanism whereby the excitatory input at E (Fig. 1) produces alterations in neurone A which result in a long-lasting increase in transmitter release at its terminals T.

The basic assumption of the quantum hypothesis of transmitter release is that the mean number of quanta released by a nerve impulse, m , depends on n , the store of immediately avail-

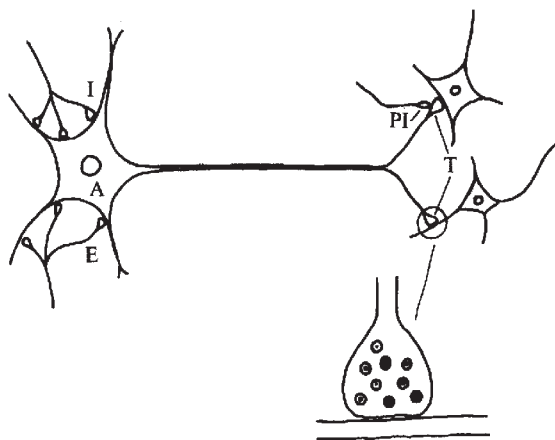


Fig. 1 Neurone A receives excitatory synaptic terminals at E and inhibitory terminals at I. Axon terminals T of neurone A are distributed to two postsynaptic neurones. One of the terminals is the postsynaptic element of an axo-axonal synapse, PI. Inset shows terminal T containing vesicles with a range of transmitter content indicated by the size of the black circles.

able quanta, and p , the probability of release². Thus

$$m = np$$

There is evidence which indicates that p is an expression of the number of Ca ions entering or combining with the membrane of the presynaptic terminal. It therefore varies with the potential difference across the terminal membrane⁸ and the concentration of ionized Ca and Mg in the medium⁴. Presynaptic inhibition, operating at PI (Fig. 1), represents a mechanism whereby rapid transient changes in transmitter release are produced by an alteration in p .

I propose that stimulation of neurones by presynaptic impulses leads to a long-lasting increase in n (the immediately available store), and thus in transmitter release. The immediately available store, n , has often been identified with the vesicles close to the axon membrane⁵, but no mechanism has been proposed for its control. My hypothesis consists of the following propositions (Fig. 1): (1) The axon terminals contain a vesicle population with a wide range of transmitter content, of which n represents the proportion of vesicles completely filled with transmitter. (2) Impulse bombardment at E leads to changes in neurone A which result in a long-lasting increase in the number of vesicles completely filled with transmitter in the axon terminals T.

Most of the evidence for this hypothesis is derived from peripheral sympathetic neurones, but, on the assumption that mechanisms of transmitter storage and release are basically similar in all neurones, this model should be applicable to other neurones. There are two lines of evidence for the postulate that the nerve terminal contains a vesicle population heterogeneous with respect to neurotransmitter content. Cell fractionation methods have shown that the vesicular noradrenaline of nerve terminals in control animals varies over a wide range⁶. This could be due to variation in the number of axon terminals in the organ (innervation density), variation in the number of vesicles in the varicosities or variation in the noradrenaline content of the vesicles. The vesicular noradrenaline can be raised above the mean control level by raising cytoplasmic noradrenaline⁶. Vesicular noradrenaline increases up to a saturation value which has a much lower coefficient of variation than the control value. This suggests that differences in the degree of vesicular filling at least partly account for differences between animals in noradrenaline content of a given organ. Different organs in the same animal differ in the extent to which this vesicular noradrenaline can be increased: it is much less for the rat vas deferens than for the rat heart (M. F. and P. R. C. Howe, in preparation). This suggests that the vesicles in the nerve terminals of the vas deferens are closer to saturation level than those of the heart. Electron microscopy provides indepen-

dent evidence for this interpretation. There is qualitative evidence for the identification of the core of small dense-cored vesicles with noradrenaline content⁷. In an attempt to provide quantitative evidence, the diameter of the electron dense core of small dense-cored vesicles was measured in different organs in various experimental conditions. The vesicle cores vary between 10 and 45 nm in diameter (M. F., *J. Physiol.*, in the press). Histograms of the frequency distribution of core diameters show a peak at 18 nm for the rat heart and a peak of 39 nm for rat vas deferens.

There are two lines of evidence which suggest that only filled vesicles can release their contents. The size of transmitter quanta has been found to fall within a narrow range wherever a true measurement of quantal size is possible^{8,9}. The contents of filled vesicles would be expected to fall within a narrow range whereas incompletely filled vesicles could have a wide range of transmitter content. Measurement of core size distribution in terminals after intense stimulation shows a loss of large vesicle cores and an increase in the percentage of small cores. This suggests a loss of filled vesicles.

If n is equivalent to filled vesicles it follows that a rise in the number of filled vesicles should be accompanied by an increase in the amount of transmitter released. There is evidence that this is the case. If vesicular noradrenaline is raised by the administration of exogenous noradrenaline and the subsequent loss of noradrenaline resulting from increased reflex stimulation is compared with that from control animals, it is found to be consistently greater¹⁰. The finding that neuronal rest increases the noradrenaline content of splenic nerve terminals and that an increased amount of noradrenaline is released from such "rested" nerve terminals also provides supporting evidence¹¹.

Finally, the induction of tyrosine hydroxylase and dopamine- β -hydroxylase by presynaptic impulse bombardment provides a mechanism for a long-lasting increase in the number of filled vesicles. In the experiments referred to so far an increase in n represented filling of an increased proportion of vesicles by raising cytoplasmic noradrenaline in one of two ways: administration of exogenous noradrenaline or inhibition of monoamine oxidase⁶. In physiological conditions the filling of vesicles must occur mainly by synthesis of noradrenaline.

The two most important enzymes involved are tyrosine hydroxylase, which is found in the cytoplasm and is the rate-limiting enzyme¹², and dopamine- β -hydroxylase, which is associated with the vesicles¹³. The rate of activity of tyrosine hydroxylase is controlled by feedback inhibition; an increased level of noradrenaline can therefore be achieved by synthesis only if the amount of enzyme is increased. This has been shown to occur in conditions of increased presynaptic bombardment¹⁴. This could lead to an increase in the proportion of filled vesicles and a consequent increase in transmitter release. It is not yet clear what happens to vesicles after they release their contents. There is some indication that, as in the case of chromaffin granules¹⁵, recently emptied vesicles in the nerve terminals may not be able to refill with noradrenaline¹⁶. If that is so, more new vesicles also need to be supplied. The rise in neuronal dopamine- β -hydroxylase, the marker enzyme for vesicles, within a few hours of the onset of increased presynaptic bombardment¹⁶ shows that this is actually taking place. In this way there should be an increase in the total number of vesicles as well as in the number of filled vesicles.

The induction of enzymes is preceded by increased RNA synthesis^{17,18}. The association of learning with increased RNA synthesis has been known for some time¹⁹, but its significance has not been understood. The increased RNA synthesis depends on the interaction of the presynaptic neurotransmitter with the postsynaptic receptor mechanism but is not dependent on the impulse traffic in the postsynaptic neurone²⁰.

A number of recent findings are combined in the present hypothesis which postulates a mechanism whereby long lasting alterations in synaptic efficiency result from increased stimulation. Some such alteration in synaptic efficiency is almost certainly involved in learning and memory but does not by

itself provide a full explanation of these phenomena. The present model has the following implications: alterations in neurone A (Fig. 1) occur as the result of the action of the transmitter released from the terminals E. These alterations take place as a result of the excitatory input at E, but do not depend on the response of neurone A which may be suppressed by inhibitory inputs at I. The alterations in neurone A result in an increase in transmitter release at all its terminals, which may be widely distributed: the potentiation will therefore affect all pathways in which neurone A participates. Finally, although the potentiation is produced by activity in terminals E, any other input which leads to the discharge of neurone A will produce the potentiated response.

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Alpha Rhythms and Hyperkinesia

In his report "Alpha Rhythms in the Hyperkinetic Child"¹, Shetty attempts to define the neurophysiological substrate of hyperkinesia by interpreting drug-induced variations in the alpha rhythms of the human electroencephalogram (EEG). Such an attempt presumes knowledge of the origin of alpha rhythms, but the single article² to which Shetty refers, and which is said to allege that "alpha rhythms represent an inhibitory process involving discharge of sub-cortical influences back into the cortex, and effecting the termination of cortical excitation associated with automatization or habituation of learned behaviour", in fact contains no evidence on such physiological processes. It refers in turn to an earlier physiological observation—the so-called "recruiting response" in cats³, which may or may not have any connexion with human alpha rhythms.

Some controversy surrounds the origin of alpha rhythms. Mulholland and Evans⁴⁻⁶ have demonstrated that the presence or absence of alpha rhythms is determined not by the presence or absence of relaxed wakefulness but by oculomotor function. Complementary observations by Lippold^{7,8} suggest that both physiological tremor and alpha rhythms in the EEG reflect oscillations in striate muscle control systems.

Some of the earliest physiological observations on alpha rhythms show a significant relationship between metabolic rate and alpha rhythm voltage⁹. In particular hypothyroidism, and the accompanying reduction in physiological tremor, has been known for many years to be associated with a reduction in the amplitude of alpha rhythms¹⁰. It is therefore unfortunate that Shetty did not make a comparison both of alpha rhythm characteristics and physiological tremor in normal and hyperkinetic children. The only conclusions which can reasonably be drawn are that central nervous stimulants such as amphetamines, which are well known to increase the magnitude of physiological tremor¹¹, also increase the alpha rhythm power range. This provides no direct information concerning the neurophysiological substrate of hyperkinesia although it does support Lippold's hypothesis that oscillation in striate muscle control systems is the source of alpha rhythm and that a tendency for overall change in muscle control systems will be paralleled by changes in the alpha signal.

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Alpha Rhythms in the Hyperkinetic Child

BECAUSE of my misunderstanding about certain technical aspects of the research which I reported recently¹ I would like to make the following clarifications.

The magnetic tape recordings from which the *r* values of Table 1 were computed were obtained under two different filter settings. Eight electroencephalograms (six patients and two controls) were recorded on f.m. tape with 10 cycle low pass filter (Grass model 78 EEG recorder), whereas nine (six patients and three controls) were recorded with 1 cycle filter. Data from twelve subjects and all the five controls were collected directly on f.m. tape. Data from sixteen patients were transferred to f.m. tape from ink writing charts for purposes of analogue to digital conversion necessary to frequency analysis. This transfer was performed by an automatic curve reader. But because of the high paper speed to the reader, considerable attenuation of high frequency components of the electroencephalogram existed in this group. Only the alpha band was selected for the analysis because this band was least affected by these manoeuvres. Moreover, the effect on alpha band was remarkable on inspection of the electroencephalogram record obtained on paper in at least one patient, and this was another reason for limiting the analysis to alpha frequencies. Thus, this band is the most sensitive, but might not be the only one which was sensitive

to the procedures described in the paper. There were some instances in which all frequency bands showed alterations.

Because of these limitations, this compilation of data may be considered preliminary and yet to be confirmed.

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Visual System's View of Acoustic Space

SINGLE nerve cells of the visual cortex of the cat can respond to stimuli in other modalities¹⁻¹¹, but the physiological role of such excitation is not understood. Many investigators consider that this activation reflects generalized arousal¹²⁻¹⁵. In this report I present evidence of a rigorous spatial specificity in the acoustical projection to visual neurones.

Each of seventeen adult cats was studied for 4-5 days for 4-10 h per day. An extracellular recording technique¹⁰ was used to monitor activity in well differentiated single units of parastriate cortex (areas 18 and 19 of Brodmann or II and III of Hubel and Wiesel). Each animal was prepared under general anaesthesia about a week before the first recording session. A 1-cm diameter trephine opening was made over the right parastriate region (gyrus marginalis, the posterior rim anterior to Horsley-Clarke zero and the medial edge within 2 mm of the midline). The dura was not removed. A nylon chamber, 1 cm high with a cap which could be opened for later insertion of the microelectrode, was placed in the trephine opening and secured with screws and dental cement. At the same time, a series of bolts and pre-drilled bars was similarly secured anterior to the chamber. These were later used to fix the cat into the stereotaxic frame so that at the time of recording the animal could be held rigidly without ear or eye bars and without pain, pressure points or open wounds of any kind. The chamber was

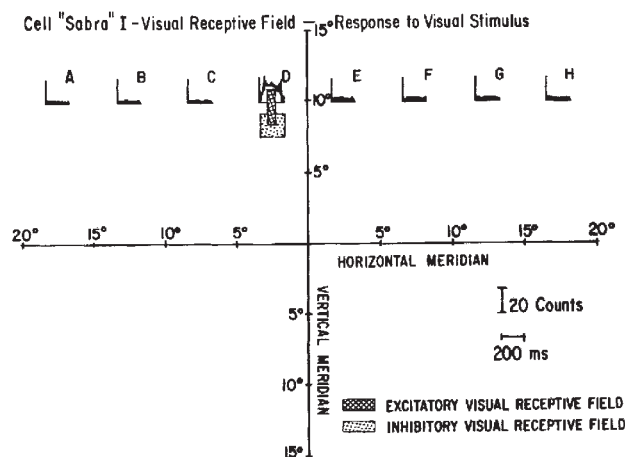


Fig. 1 Tangent screen map with superimposed post-stimulus time histograms of this particular cell's response to the "preferred" visual stimulus (a vertically oriented lighted bar with a luminance of $2.3 \log_{10} \text{ cd m}^{-2}$ (background $0.03 \log_{10} \text{ cd m}^{-2}$) extending up from an inhibitory zone). The stimulus is diagrammed at site D, the only place it affected the cell. The response pattern is shown in the histogram which is also placed at site D. The other histograms, A-C and E-H, show only a small level of background activity but nothing which was stimulus-related. All histograms were positioned on the tangent screen map at the site where the stimulus which elicited them was delivered. Analysis period: 200 ms. Bar at time zero: 20 counts. Bin width 5 ms. Histograms were sums of twenty stimulus presentations.

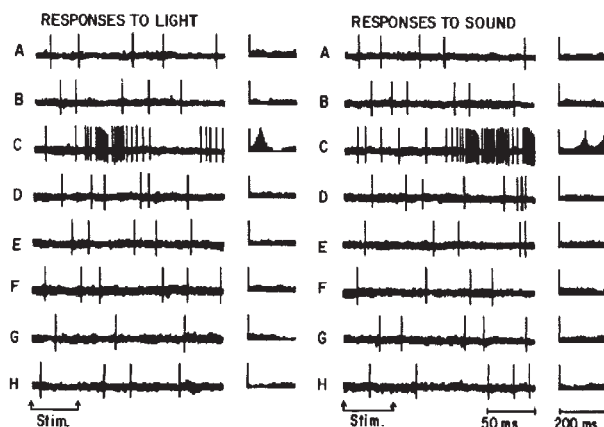


Fig. 2 Single responses and histograms of response to twenty stimuli from another cell (69-51) with a horizontal array similar to that in Fig. 1, that is, A-D in contralateral half-field, E-H in ipsilateral field. Comparison may be made of response to light (left) and response to sound (right) as a consequence of distance from the vertical meridian of the site of stimulus delivery. For both modalities excitation of the cell occurred only when the origin of the stimulus was at site C, and the response pattern was different in the two cases (see single examples at site C) and the pattern difference was stable (compare the corresponding histograms). Visual stimulus was a dark bar ($0.00 \log_{10} \text{ cd m}^{-2}$; background $0.7 \log_{10} \text{ cd m}^{-2}$) moving from right to left. Acoustical stimulus, although effective over a much larger vertical range, was also most effective when moved in a right to left direction (see Fig. 4). For acoustical stimulus arrangement, see text.

filled with a silicone gel and capped. At the time for recording, the cat was intubated, paralysed with gallamine and artificially respired. General anaesthesia could not be used because it abolishes or markedly reduces the non-visual responsiveness of visual cells¹⁶⁻¹⁸, but care was taken that the procedure should involve no stress for the animal. Body temperature was maintained at 37° C, the body was supported by elastic bands and all injections were made through an indwelling femoral cannula. Heart rate and electroencephalogram (EEG) were continuously monitored and these together with frequent observation of the right, undilated pupil served as indicators of general condition. The cat was placed in the stereotaxic frame backward so that there was an unobstructed view of a white mat projection screen 1 m from the cat's eye. The left pupil was dilated with neosynephrine, the cornea coated with methyl cellulose and a contact lens applied to correct refractive error and bring images projected on the screen into sharp focus.

Each morning for the following 4-5 days the same procedure was used. At the end of the week the animal was killed with nembutal, the brain perfused with formalin and the entire territory of all electrode tracks (each of which had at least two electrolytic lesions) was serially sectioned, stained with cresyl violet and electrode tracks reconstructed and correlated with micrometer readings taken at the time each cell was encountered and when electrolytic lesions were made.

In each experimental session, as soon as the animal was properly positioned, the chamber was opened and a tungsten microelectrode¹⁹ was inserted through the silicone gel and on into the subjacent brain. Electrical activity was recorded through Tektronix type E and D preamplifiers after a first stage field effect transistor (FET) with unity gain. Cellular activity was displayed on an oscilloscope along with Schmitt trigger pulses set to pick out easily distinguishable single units. Correspondence between the Schmitt trigger and the relevant nerve action potentials was monitored continuously and the trigger sensitivity reset for each new cell. Unit activity and Schmitt trigger pulses were recorded on f.m. magnetic tape, along with stimulus indicators, a time base and a voice channel. A continuous loud-speaker monitor was also used. Data analysis was in the form of post-stimulus time histograms generated by a LINC computer.

Each single unit (Fig. 2 illustrates a typical signal-to-noise ratio) was investigated with a variety of visual stimuli. These included slits, bars (light or dark), edges and corners, both moving and stationary and of various sizes and luminance. Our visual stimulus repertoire and the characteristic cellular responses of this region have been previously described and illustrated^{10,20}. A total of 169 cells were studied for prolonged periods (about 3 h). All the cells had well defined visual receptive fields, complex stimulus preferences and complex response patterns and conformed to the characteristics of the complex and hypercomplex cells of Hubel and Wiesel²¹. After the boundaries of the visual receptive field had been established, attempts were made to drive the cells with acoustical stimuli. The latter included tone pulses of 5–50 kHz with rise and fall times of 35 or 300 ms, click trains (1-ms clicks lasting 50 or 100 ms) and high frequency noise bursts which had a hissing sound. The sound pressure level was 30–40 dB (with regard to $0.0002 \text{ dynes cm}^{-2}$) measured at the cat's head for all stimuli. The stimulus was delivered through small (2.5 cm) focused loudspeakers mounted at various sites on the tangent screen used to project visual images, in an IAC sound-attenuating room. Acoustical stimuli were delivered in the dark or with the cat's eyes otherwise occluded or, when both acoustical and visual stimuli were presented simultaneously, the loudspeakers were masked behind a thin cheesecloth screen.

Seventy of the 169 cells were responsive to acoustical as well as to visual stimulation. Although we attempted stimulation with a range of frequencies from 5 to 50 kHz, in none of these cells could we demonstrate tuning curves analogous to those of the primary auditory system or in the few striate cells described by Spinelli *et al.*²². What seemed to determine the responsiveness of the visual cells to acoustical stimuli was the place or site of origin of acoustical stimulation. Along the horizontal meridian, the acoustical receptive fields were arranged in as orderly and systematic a way as the receptive fields for vision. In fifty-one cells, both visual and acoustical fields were contralateral to the cell examined; nineteen cells were driven from the ipsilateral field by both modalities. Cells sensitive to moving stimuli were usually selective in direction and the direction was the same for both modalities (Fig. 4).

Fig. 1 shows a tangent screen map with the post-stimulus time histograms placed at the positions of the stimulus by which they were elicited. The preferred visual stimulus (vertically oriented "tongue" extending dorsally from an inhibitory zone) is drawn near site D, that is, the only place where it was effective. Visual stimulation at all other sites in this horizontal array, that is A–C and E–H, did not elicit responses. Another cell (Fig. 2) illustrates single responses as well as the corresponding histograms using a similar horizontal array and comparing responses to light (Fig. 2, left) and to

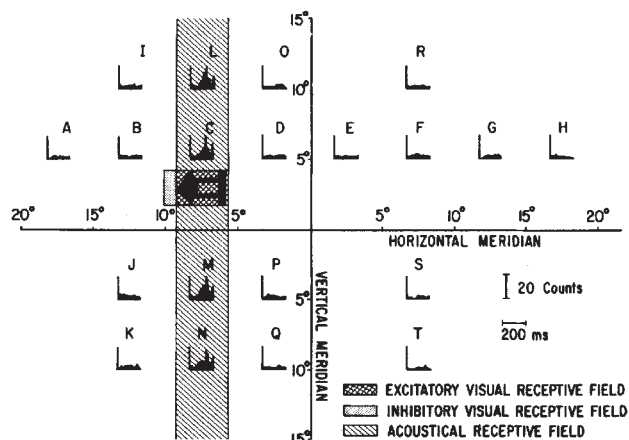


Fig. 4 Tangent screen map illustrating receptive field boundaries for both vision and audition in cell 69-51. The histograms in this case were all elicited by acoustical stimuli arising from the small focused loudspeakers placed on the tangent screen at all the lettered sites where the histogram inserts are shown. Most effective stimulation occurred when loudspeakers were moved from the medial border of the field to the lateral at about 2° s^{-1} . Most effective visual stimulation occurred when the dark bar as illustrated moved from medial to lateral border at about the same speed. Histograms are sums of twenty stimulations.

sound (Fig. 2, right) when the loudspeaker was placed at sites A to E. Thus, in the horizontal dimension the acoustical receptive field was isomorphic with the visual receptive field for that cell. For both modalities, responses were elicited only when stimulation occurred at site C. The stimulus sites were then rearranged so as to test four vertically oriented "tracks" on the tangent screen. Comparison of these "tracks", two of which are illustrated (Fig. 3, I, B, J, K and L, C, M, N), was then made for the two modalities. Again, responses occurred at site C in both cases. The acoustical stimulus, unlike the visual, however, yielded clear stimulus-related discharge at all sites in the same vertical dimension (Fig. 3, right, L, C, M, N). This pattern was characteristic of every one of the seventy cells with bimodal responses. The acoustical receptive field was thus mapped onto the receptive field organization for vision as a series of vertically oriented strips (Fig. 4) having the same horizontal boundaries as the corresponding visual receptive field but being unlimited in the vertical or altitudinal dimension (at least in the frontal plane, testing was not done above, below or behind the animal). When the loudspeaker was optimally placed, cell discharge was obtained equally well with clicks, pure tone pulses from 5–50 kHz and high frequency noise bursts.

There was no evidence of flanking or surround inhibition of acoustical receptive fields. Nevertheless, inhibition of either spontaneous or acoustically evoked discharge was seen when the inhibitory visual receptive zone was stimulated by light or when visual stimuli were moved in a direction opposite to that which was preferred, for example (from left to right in Fig. 4).

For one primary system (vision) activated by one non-primary modality (auditory), therefore, the information conveyed cannot be described as "non-specific". The only comparable observation we know of is that by Wickelgren²³ on bimodally sensitive cells in superior colliculus. Such information, derived in the superior olivary complex and sent to inferior colliculus^{24–27}, might then be relayed to superior colliculus and on to parastriate cortex. Alternatively, the information about acoustical space may be conveyed to parastriate neurones through auditory cortex^{28–30}, although a direct intrahemispheric connexion has not been demonstrated in the monkey³¹ or the cat³². But the visual association areas, that is, 18 and 19, are known to send massive corticofugal projections to superior colliculus³³ and may be responsible, at least in part, for the acoustical field organization described by Wickelgren²³. Neither proposal is exclusive; both may be operative.

Whatever the pathway, there is a certain biological sense

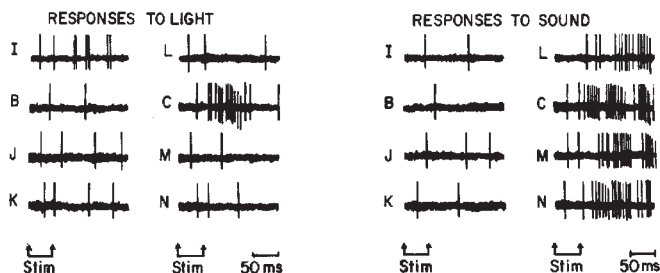


Fig. 3 Same cell as in Fig. 2. Stimuli were rearranged so as to make four vertical arrays parallel to the vertical meridian and crossing the horizontal array shown in Fig. 2. Single responses to stimuli in two of the arrays, I, B, J, K and L, C, M, N (see Fig. 4) may be compared for visual and acoustical stimuli. Visual stimulation was effective only at site C (as in Fig. 2) while acoustical stimulation was effective, not only when the loudspeaker was at site C, but from all sites in the same vertical array, that is, L, C, M, N. Thus the visual receptive field was limited in both horizontal and vertical dimensions while the acoustical receptive field exhibited the same sharp boundary in the horizontal plane but appeared unlimited in the vertical plane.

about the fact that acoustically sensitive visual cells are more concerned with spatial attributes of sound than they are with sound frequency. Because the organization of the visual system is primarily spatial and all these cells have receptor surfaces, it seems biologically adaptive that they have roughly concordant fields for sound. Such an organization might alert the visual system of an event occurring at a particular distance from the fovea and thus allow the oculomotor system to bring the area into direct foveal alignment.

The fact that the acoustical receptive fields have the shape of strips unbounded in the vertical dimension is consistent with the observation that cat, monkey and man have extreme difficulty in learning an auditory discrimination in which the only cue is location in the vertical plane (ref. 34 and personal communication from W. D. Neff). The unrestrained dog or cat, when orienting to a laterally placed sound, not only turns but invariably tilts the head, thereby adding a horizontal dimension to vertical space.

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Improved Relationship between the Deposition of Strontium-90 and the Contamination of Milk in the United Kingdom

ONE of the main difficulties in estimating the integrated radiation dose which the population may receive from environmental contamination with strontium-90 has been to determine the average extent to which it enters food chains many years after deposition. Environmental surveys gave little information on this question in the earlier years of fallout from nuclear weapons tests as ⁹⁰Sr directly retained on vegetation was then the main source of dietary contamination¹. A more complete analysis was possible after 1966 when the reduced rate of fallout caused uptake from the soil to make the dominant contribution. The integrated total of ⁹⁰Sr which will enter diet from a given deposition now appears to be about half that suspected a decade ago.

On the simplest analysis, the ratio of ⁹⁰Sr to calcium in milk, which is a suitable index of the contamination of the

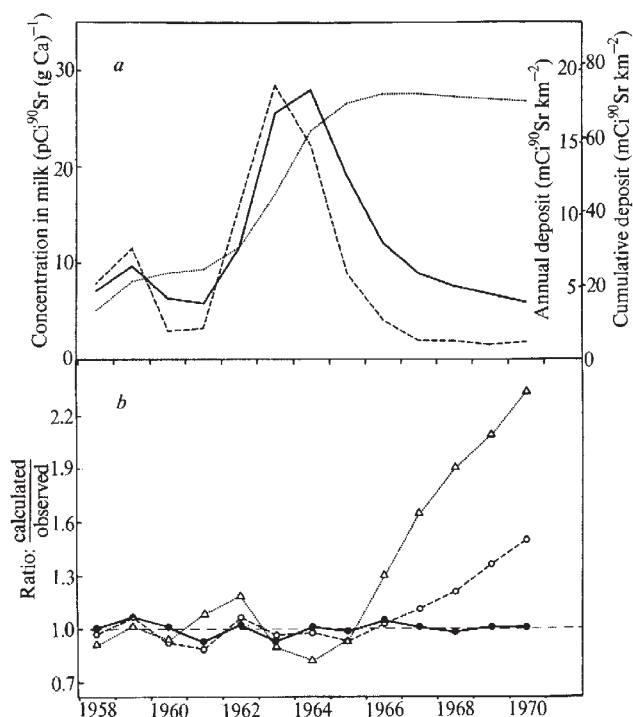


Fig. 1 Strontium-90 in fallout and milk in the United Kingdom, 1958-1970. a, Mean results of surveys of deposition and contamination of milk conducted respectively by the Atomic Energy Research Establishment, Harwell⁷, and Letcombe Laboratory. —, Milk; . . . , cumulative deposit; - - -, annual deposit. b, Annual average ratios of strontium-90 to calcium in milk, calculated by alternative equations, expressed relative to the observed values. Δ . . . Δ , Equation (1) (1961); $\circ$ - - - $\circ$, equation (2) (1964); $\bullet$ — $\bullet$, equation (3) (1969). The dates in parentheses indicate the most recent information available when the proportionality factors for the equation were derived.

total diet in western countries², consists of two components proportional respectively to the recent deposit which can lodge on vegetation and to the cumulative deposit in soil. Thus in 1958 the United Nations Scientific Committee on the Effects of Atomic Radiation (UNSCEAR)³ used an equation of the following type to relate the annual mean contamination of milk to measurements of fallout:

$$C = p_r F_r + p_d F_d \quad (1)$$

where C is the annual mean ratio of ^{90}Sr to calcium in milk ($\text{pCi } ^{90}\text{Sr (g Ca)}^{-1}$); F_r and F_d are respectively the deposit of ^{90}Sr in the year in question and the cumulative total at mid-year allowing for radioactive decay (mCi km^{-2}); and p_r and p_d are proportionality factors. The processes involved were known to be more complex than the equation implied⁴ but a more adequate analysis was impossible until survey results were available for an extended period.

Surveys of the deposition of fallout in the UK, conducted by the UK Atomic Energy Authority, and of the contamination of milk, measured in this laboratory, up to 1961 gave values of 0.76 and 0.19 for p_r and p_d ⁵. Concentrations in milk thus calculated agreed reasonably with those observed for each year until 1964 (Fig. 1) but a consistent discrepancy was apparent. The contamination was overestimated when fallout had been comparatively low in the previous year, the reverse being true when it was high. This confirmed an anticipated defect in equation (1). In addition to entering foodstuffs from the soil, fallout in the previous year directly contaminates stored cattle food; moreover a fraction may be retained by the basal parts of plants¹. A revised equation took account of this "lag effect" which was more closely related to the deposit in the last half of the previous year than to that in the whole year or in the summer only⁶:

$$C = p_r F_r + p_1 F_1 + p_d F_d \quad (2)$$

where F_1 is the deposit in the last half of the previous year and p_1 the "lag-rate" factor, the other symbols being as in equation (1).

Table 1 Comparison of Observed and Calculated Annual Mean Ratios of ^{90}Sr to Calcium in Milk in the UK

Year	Deposition of ^{90}Sr (mCi km^{-2})	Contamination of milk		
		Observed (pCi (g Ca)^{-1})	Calculated (equation (3)) as % of observed	% attributable to uptake from soil
1958	5.3	7.2	100	17
1959	7.7	9.8	106	16
1960	7.0	6.4	101	42
1961	2.2	5.9	93	50
1962	10.5	11.7	102	21
1963	19.0	25.6	93	13
1964	14.8	28.0	100	18
1965	5.9	19.0	98	38
1966	2.7	12.1	104	64
1967	1.3	8.8	101	76
1968	1.3	7.6	98	80
1969	1.0	6.8	101	76
1970	1.2	6.1	101	74

Survey results up to 1964 gave the following values: $p_r = 0.70$, $p_1 = 1.13$ and $p_d = 0.11$. Although the value of p_r differed little from that derived in equation (1), p_d was lower by some 40% and calculated concentrations in milk agreed more closely with those observed. Agreement remained reasonable until 1966 (Fig. 1). Thereafter, when the rate of fallout was low and the soil became the dominant source of ^{90}Sr in diet (Table 1), both equations led to consistent and increasing overestimates. It was thus evident that absorption from the soil had been overestimated.

This had been anticipated. The true value of p_d cannot be constant as both equations assumed, largely because removal

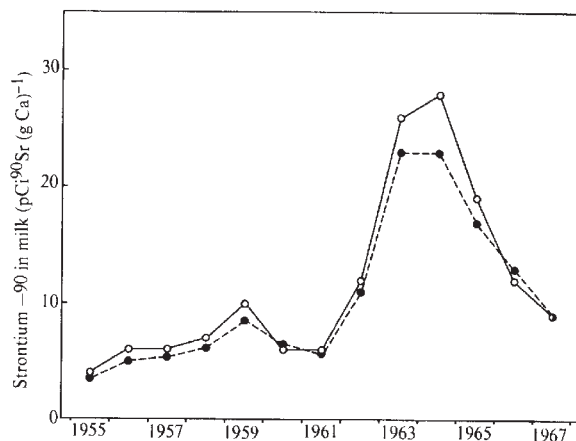


Fig. 2 Mean ratio of strontium-90 to calcium in milk in the United Kingdom (○) and in areas within the north temperate zone (ref. 11) (●), 1955–1967.

by plants, slow leaching down the soil profile, and sometimes cultivation, reduce its availability to roots until finally it passes out of their reach¹. As a first approximation it was assumed that the combined effect of these processes might lead to an exponential decrease in uptake from the soil. The best fit was obtained when the exponential function was applied to the period ending at the middle of the preceding year; this agreed with the earlier evidence of the "lag" effect in the last half of that year. The following equation was derived:

$$C_n = p_1 F_n + p_2 F_{n-1,b} + p_3 [F_{n-1,a} \exp(-1.25k) + F_{n-2} \exp(-2k) + F_{n-3} \exp(-3k) + \dots] \quad (3)$$

where C_n is the annual ratio of ^{90}Sr to calcium in milk ($\text{pCi } ^{90}\text{Sr (g Ca)}^{-1}$) in year n ; $F_n, F_{n-1}, F_{n-2}, \dots$ are the deposits of ^{90}Sr in year n and each previous year, after correction for decay to mid-point of year n (mCi km^{-2}); the subscripts a and b denote the first and second halves of year $n-1$; p_1, p_2 and p_3 are proportionality factors, and $\exp(-k)$ gives the annual decrease in the uptake of ^{90}Sr from the soil, after allowance for radioactive decay. The first two terms on the right hand side of equation (3) differ slightly from those in equation (2) as they reflect the total effect of fallout at the times indicated, including the small contribution from the soil which is included in the third term of equation (2).

Analysis of survey results up to 1969 (see refs. 7, 8, and a personal communication from R. S. Cambray) gave the following values for the constants in equation (3): $p_1 = 0.70$, $p_2 = 1.41$, $p_3 = 0.20$ and $\exp(-k) = 0.86$ (ref. 9); accordingly, uptake of ^{90}Sr from soil decreases by 14% annually. The method of calculation has been described elsewhere¹⁰. Fig. 1 and Table 1 show that this procedure gave much improved estimates of the concentration of ^{90}Sr in milk in individual years; the same was true in 1970, the maximum discrepancy in the period 1959–1970 being 7%.

Comparable analyses cannot be made for most countries because of insufficient data, but results assembled by UNSCEAR¹¹ (Fig. 2) show that the contamination of milk in the United Kingdom is highly correlated ($r = 0.99$) with the average from other countries in similar northern hemisphere latitudes. This suggests that relationships in the UK should be a reasonable guide to those elsewhere. It should not be assumed, however, that the exact form of equation (3) is the most suitable for other areas; the "lag" contribution may vary depending on animal feeding practices and the state of pastures during winter.

Modifications to take account of local situations should not, however, have a large effect on calculations of the integrated total of ^{90}Sr which will enter milk over an extended period from a given deposit. The estimation of this quantity is of crucial importance in deriving the dose commitment which UNSCEAR¹² judges to be the best index of the significance of environmental contamination. UNSCEAR¹²⁻¹⁴ formerly

used a procedure similar to equation (1), assuming in addition that the uptake of ^{90}Sr from the soil decreased by 2% per annum apart from radioactive decay. This assumption, described as "adequately cautious", had no factual basis and it was recognized that the absorption of ^{90}Sr from soil decreased more rapidly in some areas. UNSCEAR's most recent assessment¹¹ retained this assumption in a revised method of calculation. In contrast equation (3) in the present work indicates a decrease of 14% annually which differs little from 13% per annum estimated by van der Stricht *et al.*¹⁵ in northern Italy and a recent estimate of 11% in New York State¹⁶. No conflicting evidence has been traced.

The effect of this revision is considerable. On the basis of equation (1), and assuming a 2% reduction of uptake from the soil annually (which was UNSCEAR's earlier procedure) the deposition of 1 mCi ^{90}Sr km⁻² would cause the integrated contamination of milk to be 4.9 mCi ^{90}Sr y⁻¹ (g Ca⁻¹) in the UK. The corresponding figure derived from equation (3) is 2.3; this halves the estimated dose commitment. Moreover, whereas the earlier analysis suggested that absorption from the soil accounted for 84% of the integrated total, equation (3) reduces the figure to 47%.

Despite the close description equation (3) provides of past experience, it remains empirical, especially the form of lag component and the assumption of exponential decrease in uptake from the soil. Moreover the equation is mathematically less elegant than if only continuous-time functions were employed. The limitations of the survey data, however, made a more refined treatment seem unrealistic and confidence is encouraged by the fact that as more information became available it was possible to take account of recognized defects in earlier approaches.

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Resistance to Synthetic Juvenile Hormone in a Strain of the Flour Beetle, *Tribolium castaneum*

INSECT hormones, and compounds which mimic their effects, have potential as insect control agents. Substances with juvenile hormone activity have considerable promise in this respect, and it has been suggested that insect strains resistant to such compounds would be unlikely to evolve^{1,2}. The present study shows that a strain of *Tribolium castaneum* resistant to many conventional insecticides is also resistant to juvenile hormone.

The susceptible strain used has been in culture since before 1950, and the resistant strain was strain CTC12 of Champ and Campbell-Brown³. The latter is known to be resistant to DDT, lindane, bromodan, and all four carbamate and twenty-two organophosphorus insecticides tested against it³⁻⁵. The synthetic juvenile hormone used was a *cis/trans* mixture of methyl 10,11-epoxy-7-ethyl-3,11-dimethyl-2,6-tridecadienoate.

Batches of twenty-five larvae, reared on wholewheat flour, were transferred to jars of similar food containing various levels of the hormone when 14-15 days old. Thereafter they

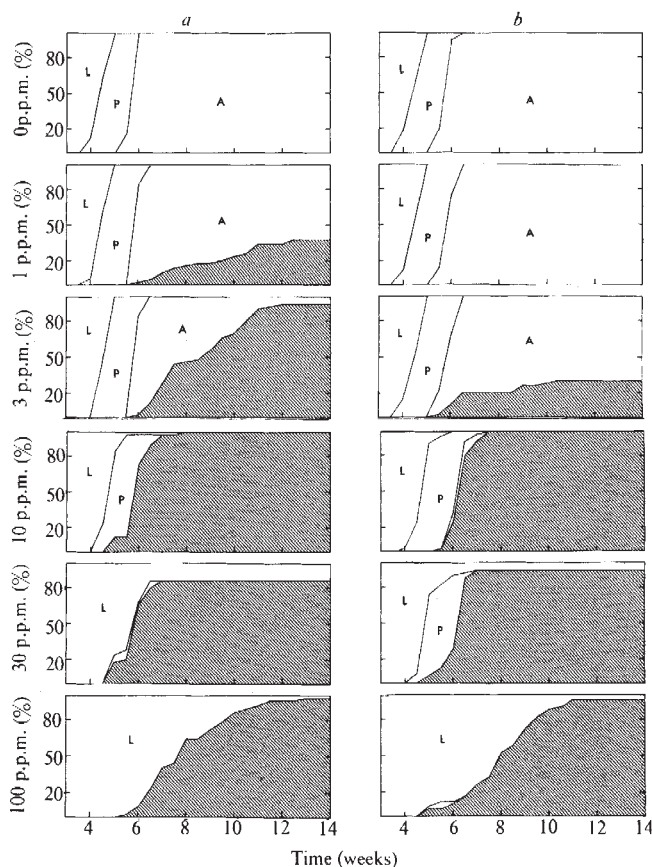


Fig. 1 Mortality and metamorphosis in larvae of (a) susceptible and (b) resistant strains of *Tribolium castaneum* added to flour containing various levels of juvenile hormone. The shading shows the increase of mortality (in all stages) with time. L, Living larvae; P, living pupae; A, living adults. Percentages are based on fifty insects.

were maintained at 25° C, 75% relative humidity and sieved off, examined, and returned to the same jar, twice each week. The stages present and mortality were recorded. No attempt was made to classify degrees of "juvenilization" or pupal deformity. The adult stage was considered to begin when the pupal cuticle had been completely shed, and insects which died during this ecdysis were classed as dead pupae.

At 0.3 p.p.m. the treatment had no apparent effect on either strain. Other results are shown in Fig. 1. At 1 and 3 p.p.m. mortality was predominantly in adults after successful metamorphosis, though in the resistant strain 1 p.p.m. had no apparent effect, and the mortality at 3 p.p.m. was much reduced. At 10–100 p.p.m. the overall mortalities were similar in the two strains, but, because of different effects on the success and timing of metamorphosis, the stages which died were different (Table 1). Compared with the susceptible strain, the resistant strain produced more adults before death at 10 p.p.m., and more pupae before death at 30 and 100 p.p.m. At 30–100 p.p.m. some insects survived as larvae in both strains, and many of these were abnormally large as has been reported in other insects. At 30 p.p.m. pupation was delayed in the susceptible but not in the resistant insects.

Table 1 Stage Reached and Mortality in Susceptible (S) and Resistant (R) Strains of *Tribolium castaneum* after 12 Weeks in Flour treated with Juvenile Hormone

Juvenile hormone (p.p.m.)	Strain	Live larvae (%)	Dead larvae (%)	Dead pupae (%)	Dead adults (%)
10	S	0	2	98	0
	R	0	0	82	18
30	S	14	48	38	0
	R	6	12	82	0
100	S	2	98	0	0
	R	4	82	14	0

By increasing the dose of juvenile hormone, adult mortality, pupal mortality, delay in pupation, prevention of pupation, and larval mortality were produced in both strains. A larger dose was required for each of these effects in the resistant strain, but because of variation in the average age at which these effects occur, mortality at any particular time is not a simple function of dose. Thus whereas in both strains 10 p.p.m. gave 100% mortality, at 30 p.p.m. some insects seemed to be protected from pupal death by failure to pupate.

The practical development of pesticides based on juvenile hormone will have to take into account their total effects. With *T. castaneum* these include adult mortality after successful metamorphosis (which would be missed in assays measuring only degrees of metamorphosis) as well as the damage potential of abnormally large larvae produced at higher doses. The occurrence of strains resistant to the hormone must also affect the practical utility of these compounds. Many of them, however, have high selectivity compared with conventional insecticides. Low toxicity to vertebrates may therefore permit the use of higher dose levels to combat resistance if this should prove necessary.

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Statocyst lacking Cilia in the Coelenterate *Corymorpha palma*

STATOCYSTS are sensory organs which detect the direction of gravitational force, and usually consist of a vesicle which contains a nodule (statolith) resting on a bed of sensory hairs (generally cilia). Changes in orientation cause the statolith to roll onto and stimulate new groups of hairs which pass information to the nervous system. This pattern is true of most coelenterate statocysts¹. I wish to describe a coelenterate statocyst in which cilia and nerves are absent, the statolith resting directly on the wall of its vesicle.

Corymorpha palma, the "fairy palm", is a solitary hydroid

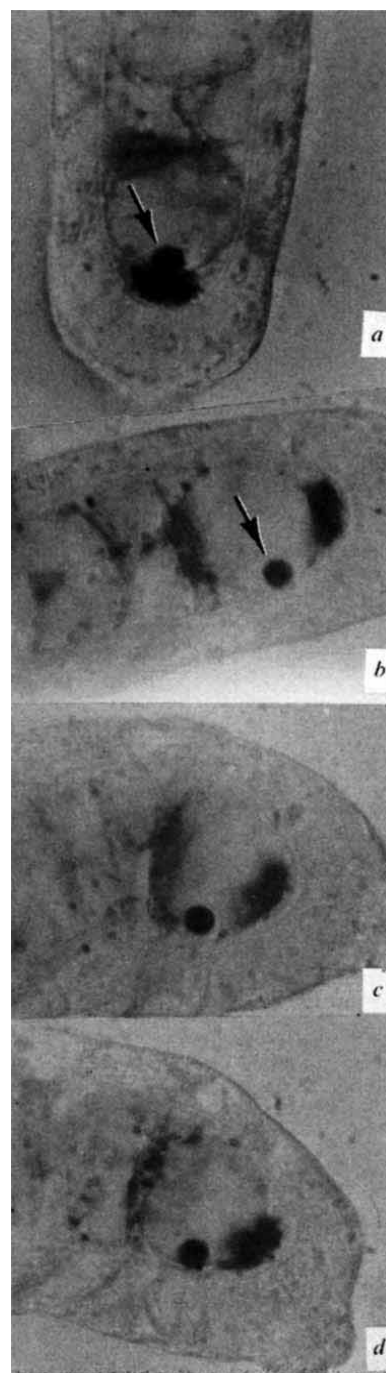


Fig. 1 The response of the holdfast to rotation. Arrows indicate the statolith: a, 1 min before rotation; b, 4 s after rotation about 120°; c, 13 s after rotation; d, 30 s after rotation. The dark mass in the terminal endodermal cell is an aggregate of refractile bodies similar to those in the statolith. (Nomarsky contrast interference, $\times 700$.)

polyp which lives on marine mud flats. Its transparent stalk is several inches long and is anchored by tissue filaments extending into the mud. These filamentous holdfasts are stolon-like outgrowths of the body wall, but, unlike typical hydrozoan stolons, each has a statocyst at its tip². *Corymorpha* statocysts, unlike most statocysts which are deeply buried in tissue, are suitable for study *in vivo*. A polyp placed on a microscope slide (after the thin perisac is stripped off) quickly produces new holdfasts which elongate along the glass surface. The polyp may then be removed. Holdfasts adhering to the glass will continue to elongate for hours and may be covered with a raised coverglass for all types of critical light microscopical study. Observations on such living preparations were made using a horizontal microscope with a vertical rotatable stage. Holdfasts were fixed for 2 h in 4% glutaraldehyde with 1% acrolein in seawater containing 0.1 M cacodylate buffer, pH 7.4, postfixed for 1 h in 1% osmium tetroxide, embedded in 'Epon', and studied by light and electron microscopy.

The holdfast tip (Figs. 1a and 2a) consists of a column of endoderm cells, a thin layer of mesogloea, and a covering of ectoderm. The spherical or ovoid statocyst vesicle is a cavity between the two terminal endodermal cells and is bounded only by the plasma membranes of these cells. The nucleus and most of the cytoplasm of the terminal cell are distal to the vesicle; only a thin layer of cytoplasm extends proximally, separating the vesicle from the mesogloea. The junction between this cup-shaped cell and the next cell lies near the back surface of the statocyst (Fig. 2a, b). The lateral sheath of the distal cell is very thin, as observed with the light and electron microscopes.

The statolith (Fig. 2c and d) has the appearance of compacted cell cytoplasm. It is about 5 μm in diameter and may be round or somewhat irregular in shape. Its most conspicuous components are ovoid bodies, about 1 μm long, which are refractile and birefringent and presumably heavy. Between them are numerous mitochondria and the statolith is bound by a unit membrane. Birefringent bodies and mitochondria similar to those in the statolith are present in many endodermal cells of the holdfast; they are particularly abundant in the two terminal cells. It seems likely that the statolith arose from one of the terminal cells.

During holdfast elongation the tip migrates across the substratum or through the mud, stretching the trailing filament². No cell division occurs during this process. The tip migrates and responds to gravity even after it is severed from the trailing holdfast.

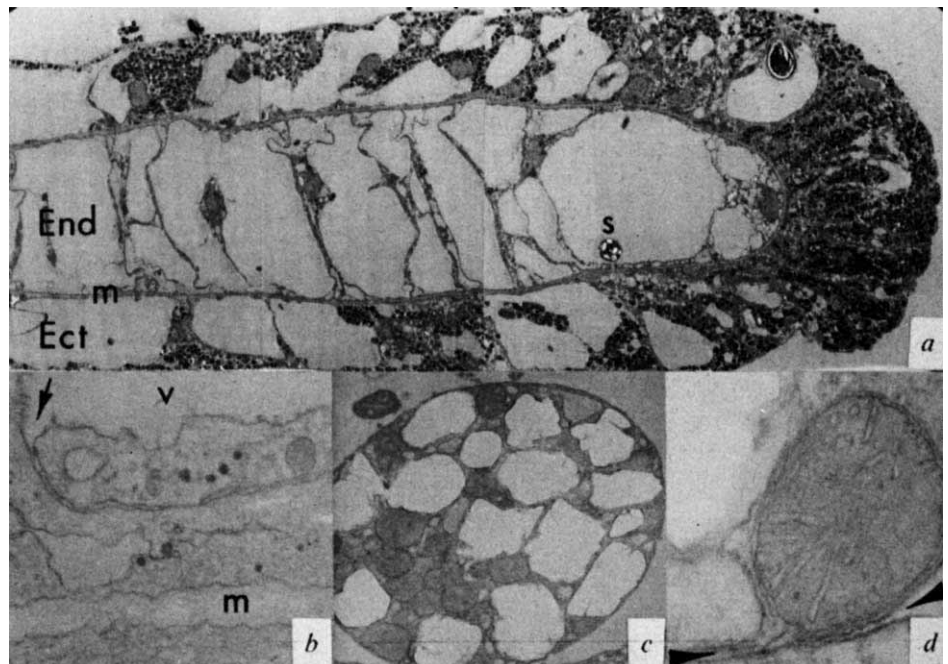
When a holdfast tip is directed downwards (Fig. 1a), the statolith rests at the distal end of the statocyst. If the holdfast is rotated so its tip is no longer pointed downwards, the statolith gently tumbles against a lateral statocyst surface where the enveloping cell is very thin. A few seconds later, the tip of the stolon contracts and bends toward the side where the statolith is resting (Fig. 1b-d). Bending is due to local contraction of the tissue adjacent to the statocyst, particularly on the side of the statolith (generally the entire filament tip retracts and thickens slightly during the first minutes, as shown in Fig. 1c). Contraction is restricted to the terminal 50–75 μm of the holdfast. The migration of the tip continues in the new direction in which it is pointing, and so it eventually becomes vertical.

The statolith rests directly on the membrane of the cup-shaped vesicle cell (Fig. 2c and d). No cilia are present; this is concluded from oil immersion light microscopy (Brightfield and Nomarski interference) of holdfasts adhering to coverglasses, and from electron microscopical examination of more than thirty statocysts after various fixatives, sectioned along the three major stolon axes. Two statocysts were serially sectioned for electron microscopy in a medial orientation and at least every fifth section of one half of each was examined carefully for cilia.

The bases of the ectodermal cells are slightly interdigitating longitudinally and the cytoplasmic texture of these processes is fibrous (Fig. 2b), resembling muscular processes found in this position in many coelenterates^{3,4}. If these are the contractile elements, the stimulation apparently is transmitted through the narrow band (see Figs. 1 and 2a) of endodermal cytoplasm surrounding the vesicle. The apparent lack of stimulation when the nodule rests at the distal tip of its vesicle could thus be due to the great thickness of the endodermal cytoplasm here, insulating the sensory system from the mesolamella and ectoderm.

The transfer of information from the statocyst to the effector (contractile) system is probably not mediated by nerves. No nerve cell nuclei are present in the holdfast tip. Also, the isolated tip shows normal gravity response and migration², suggesting that the statocyst is not innervated by distant nerve cells. There seems to be no need for nerve cell conduction of information, since the effector activity occurs within a few microns of the statocyst. The lack of a nervous conducting system may be related to the lack of cilia which normally act as sensory organelles for nerve-connected sensory cells.

Fig. 2 a, Electron micrograph of a horizontal holdfast taken in a vertical section: s, statolith; m, mesolamella; Ect and End, ectoderm and endoderm. The junction between the terminal and subterminal endodermal cell is in the vicinity of the statolith in this section ($\times 756$). b, Junction (arrow) between the terminal and subterminal endodermal cells, seen in the same orientation as shown in Fig. 2a. This area corresponds to the region just below the statolith in Fig. 2a, but in a different holdfast. This shows the extracellular position of the statocyst vesicle (v). The large vesicles, visible in Fig. 2a, proximal to the statocyst are all intracellular vacuoles: m, mesogloea. The basal portion of the ectoderm, here seen below the mesogloea, has a fibrous texture and may be the site of contraction ($\times 14,400$). c, The statolith, resting on the inner surface of the terminal endodermal cell (bottom), is composed mainly of crystalline particles, which are not preserved during sectioning, and mitochondria ($\times 8,320$). d, Enlarged region of contact (arrows) between statolith (top) and endodermal cell (bottom), showing a mitochondrion ($\times 28,800$).



On the whole, coelenterate gravity-detecting organs are restricted to the medusoid generations; until now only a few, aberrant polyps were known to have statocysts (such as the Narcomedusan polyps, *Halammohydra* and *Otohydra*⁵) and no polyps in the Hydroida were known to possess them⁶. (Salvini-Plawen⁶ has described gonophore-like structures in *Siphonohydra adriatica* which may represent statocysts and B. Swedmark studied similar organs in *Euphyra aurata* (personal communication). In both cases, the probable statocysts seem similar to those in *Corymorpha*, although they are situated just below the proximal tentacle whorl. Probably all species of *Corymorpha* and the related genus *Branchiocerianthus* have statocysts in the holdfast filaments.) Statocysts are considered diagnostic in distinguishing between medusoid representatives of the Calyptoblastea, which possess statocysts, and those of the Gymnoblastera, which do not³. *Corymorpha* belongs to the suborder Gymnoblastera. The peculiar structure and morphological position of the statocyst suggest, however, that it may not be homologous with the ciliated⁷ medusa organs which are known, and certainly does not fit into the coherent evolutionary pattern of medusoid statocysts carefully analysed by Horridge¹. This statocyst resembles some of the endodermal gravity detecting organs of the primitive Trachylina except that those have sensory cilia⁷.

This statocyst thus has a number of features which raise interesting zoological and cytological questions. It is ideal for developmental studies since all stages are ordered along the polyp base at all times². Because it is so easily obtained and handled under conditions which allow extended high-magnification observations on active tissue, this statocyst may be a valuable sensory system for further experimental and cytological investigations.

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Cognitive Contours

It is surprisingly easy to devise simple line figures which evoke marked illusory contours. Unlike the well known brightness contrast effects, these illusory contours can occur in regions far removed from regions of physical intensity difference; and they can be orientated at any angle to physically present contours. Fig. 1 is the figure described by Kanizsa¹. An illusory triangle is observed whose apices are determined by the blank sectors of the three disks. The "created" object appears to have sharp contours which bound a large region of enhanced brightness.

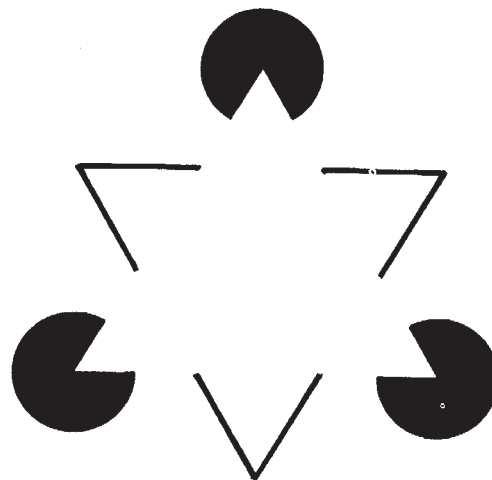


Fig. 1 Illusory triangle with apices in the blank sectors of the disks.

We can discover what features are necessary for producing this effect by removing parts of this figure (Fig. 2a-c). Three dots spaced as the apices of an equilateral triangle (Fig. 2a) give no created contours, although they are readily seen as indicating a triangle. The broken triangle (Fig. 2b) does not evoke the figure (except perhaps slightly after the effect has been observed in Fig. 1): but combining the equilaterally spaced dots with the broken triangle (Fig. 2c) does evoke the illusory object, though less markedly than with the sectorized disks of Fig. 1.

The effect has particular theoretical interest, for it could be explained by two equally plausible but very different explanations, or paradigms to use T. S. Kuhn's² term. It might be described in physiological or in cognitive terms. The physiological paradigm would be satisfied with the supposition that feature detector cells of the striate cortex³ are activated by the disk sectors (or less effectively by the dots) to give the

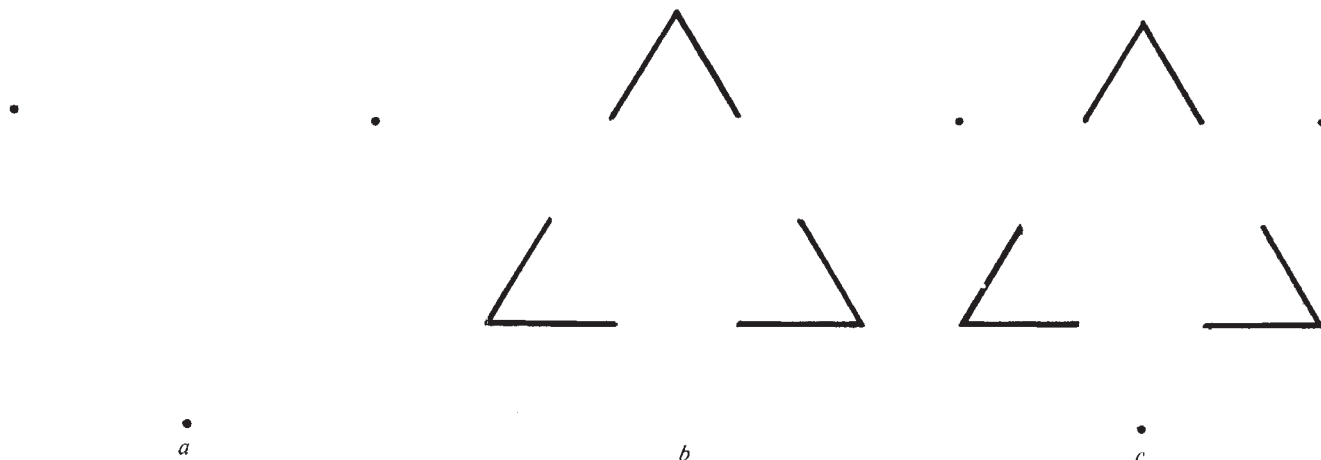


Fig. 2 a, Three dots which can be seen as indicating a triangle but produce no illusory contours. b, A broken triangle which again evokes no illusory image. c, A combination of the three dots in a and the broken triangle of b, which does evoke an illusory triangle.

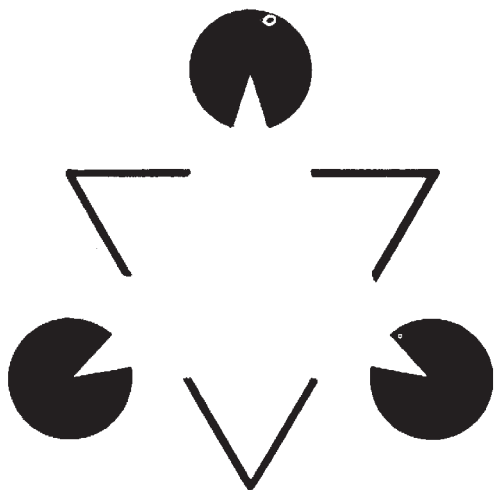


Fig. 3 Illusory triangle with curved contour.

appearance of continuous lines, though only their ends are given by stimulation. The cognitive paradigm of perception regards perceptions as hypotheses selected by sensory data, but going beyond available data, to give "object hypotheses"⁴. This paradigm would be satisfied by supposing that the illusory object is "postulated" as a perceptual hypothesis to account for the blank sectors and the breaks in the triangle. As these features are removed from the figure, the hypothesis becomes weaker, until the postulated masking object is no longer seen.

The effects also occur for stabilized retinal images (flash after-images) and so are not dependent on eye movements. By adopting the technique devised in 1899 by Witasek⁵ of sharing parts of the figures between the two eyes, with a stereoscope, it is easy to show that the effect is not retinal in origin but must be after the level of binocular fusion; the effect holds when the sectorized disks and the interrupted line triangle are viewed by separate eyes, and are stereoscopically fused. The illusory triangle may, on the second paradigm, be called a cognitive fiction—but is this notion science fiction?

By changing the angles of the disk sectors, so that they no longer meet by straight line interpolation, we find that the effect still occurs. The created form is now changed, to give interpolation with a curved fictional contour (Fig. 3). This new effect seems to increase the plausibility of the cognitive fiction notion, for it seems unlikely that "curved edge" detectors would be selected by the mis-aligned sectors. This might be answered by direct electrophysiology.

The black line on white background figures give a homogeneous whiter-than-white fictional region. The corresponding negative white line on black background gives a blacker-than-black region. The illusory intensity can be measured, in either case, with a reference light spot as in a matching photometer. Both the black and white illusory triangles are reported as appearing somewhat in front of the rest of the figures, which is compatible with the cognitive paradigm.

Viewed as flash after-images, the created regions change from black to white and vice versa whenever the after-image changes positive to negative, as for equivalent optical reversals of the display.

Not only contours but large homogeneous areas of different brightness are created—but are such areas of different brightness created by line detectors? Consider Fig. 2b. We have lines with gaps; but there is no observed difference in brightness between the inside and the outside of this triangle, and no contours between the gaps. So why should there be contours and a brightness difference with the illusory triangle, if both it and the normal line triangle are neurally signalled in the same way? The lack of contour in the gaps of Fig. 2b and the absence of enhanced brightness in such figures show that aligned features are not sufficient for producing these

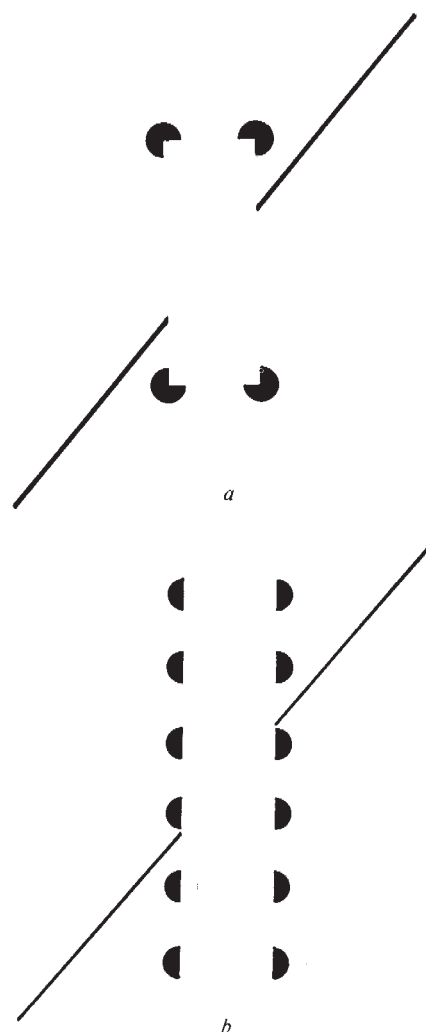


Fig. 4 *a* and *b*, Poggendorff figures generated from "cognitive contours".

effects. What seems to be needed is a high probability of an over-lying object, giving gaps by masking. This would require processes of a logical sophistication beyond those believed to occur at the striate region. A description of processes postulating objects from sensory evidence seems to require concepts beyond those of classical physiology—cognitive concepts.

Finally, we find that at least some of the classical distortion illusions can be generated by these apparently cognitive contours and regions. Fig. 4a shows a kind of Poggendorff figure, in which the usual parallel lines are physically absent, but are generated by four sectorized disks placed well away from the interrupted oblique figure. Figures such as Fig. 4b also evoke apparently cognitive contours, and they also produce distortion illusions. Such distortions in the absence of physical contours show that these illusions must, if the above interpretation is correct, have their origin at a cognitive level, which is consistent with a theory of the illusions already proposed⁶.

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BOOK REVIEWS

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The Earth's Age and Geochronology. By D. York and R. M. Farquhar. Pp. viii+178. (Pergamon: Oxford and New York, February 1972.) £2.50.

The Structure and Physical Properties of the Earth's Crust. Edited by J. G. Heacock. Pp. x+348. (American Geophysical Union: Washington DC, December 1971.) \$19.

DURING the past twenty years, the study of the permanent (remanent) magnetism of rocks has developed into a powerful new physical tool for studying the Earth and its history, comparable in importance with the study of radioactive isotopes in rocks. It has been shown that even rocks of early pre-Cambrian age can, under favourable circumstances, retain much of the magnetization they gained at the time of their formation. Rocks can act as fossil magmatic compasses, but the positions of the magnetic poles, except in geologically recent times, have apparently deviated considerably from their present position of approximate coincidence with the geographical poles. Further, there have been episodic reversals of the magnetic polarity, which are now providing a new secondary means of calibrating the geological time scale. Dr Tarling works in one of the leading schools of palaeomagnetism at the University of Newcastle-upon-Tyne. In his book, *Principles and Applications of Palaeomagnetism*, he gives an excellent account of the physics of rock magnetism and of the problems of measuring magnetic properties of rocks and relating them to the geological setting of the rocks. This comprises more than half of the book. The remaining three chapters deal with applications. While these do give overall a fairly complete coverage, the attempt to compress all the topics into fifty-nine pages leads to some sketchiness, which is particularly acute in the short chapter on geological applications. This is partly offset by a useful selected bibliography. All in all, however, the book is very good value for money for any geologist or geophysicist.

It is, of course, the dimension of time that gives geology so much of its fundamental significance. Until the twentieth century, however, the only time scale was the relative, though very important, one provided by stratigraphy. The discovery of radioactivity opened up the possibility of measuring the absolute ages of rocks, minerals and artefacts, and this was given a tremendous boost by the enormous and sophisticated development of atomic physics in the 1940s and 1950s. The past 20 years have seen an exponential rise in the number of radiometric age measurements and corresponding progress in interpretation and application of the data. York and Farquhar devote seven chapters of their book to a critical discussion of the principles and methods of radiometric dating and a further five chapters to the results and their applications. It is slightly surprising to find no references in the book dated later than 1969, which detracts from the value of the short chapter on reversals of the Earth's magnetic field. There is an interesting chapter, "The Pulse of the Earth", concerned with the history of igneous activity; but surprisingly there is no discussion of applications of radiometric measurements in palaeontology. There are also useful chapters on meteorites and the age of the Earth. Through radiometric dating, the time dimension is now becoming increasingly important in geophysics, and, in conjunction with studies of rock magnetism and with studies by the classical geological methods, it is providing the framework for a theory of the evolution of our planet. This book will be a useful addition to the library of any geologist or geophysicist.

Volume 14 of the very useful monograph series of the American Geophysical Union consists of twenty-six papers by research workers in the USA and is based on the proceedings of a symposium held at the University of Colorado in July 1970. The meeting was sponsored by the Office of Naval Research (ONR), the University of Colorado, and the Environmental Research Laboratories of the National Oceanographic and Atmospheric Administration (NOAA). It was initiated by the editor "with a special view toward answering fundamental environmental questions about the nature of

the Earth's crust", although this was linked also with the practical question of the possible use of a part of the crust as an electromagnetic wave guide for radio communications. The book is divided into sections dealing with continental crust, oceanic crust, laboratory measurements, and electromagnetic wave propagation in the crust. The physical properties dealt with are seismic wave propagation, electrical resistivity and heat flow. Half of the papers (and more than half of the books) are devoted to continental crust studies, most of them in the USA. There are useful reviews of a number of topics. These include: reviews of evidence for low seismic velocity crustal layers by M. Landisman, S. Mueller, B. J. Mitchell, and J. H. Healy; a review of evidence on low-resistivity crustal layers by H. Porath; an attempt at the construction of a crustal resistivity model to 40 km depth, based on laboratory studies, by W. F. Brace; and papers on water in the crust by P. J. Wyllie, and on the composition, structure, and evolution of the crust by A. L. Boettcher. A theme which recurs throughout the book is the growing interest in the significant role of any aqueous solutions trapped in pores in rocks at depth. Although, as concluded by the author, long range radio communication through the crust is probably not feasible, the symposium built around the problem has proved to be usefully stimulating. Because of its price and its specialized nature, the symposium volume will probably not be bought by many individual research workers, but it will surely be included in any geology and geophysics reference library.

S. A. F. MURRELL

Using the Land

Agricultural Geography. By W. B. Morgan and R. J. C. Munton. Pp. xii+175. (Methuen: London, 1971.) £2 cloth; 90p paperback.

GEOGRAPHY, as commonly defined, comprises a blend of physical, human and economic branches, so in agricultural geography a reader might anticipate that all these three aspects would be appropriately covered.

Agricultural geography as interpreted in this book embraces the systematic study of the location and distribution of agricultural enterprises giving special emphasis to approaches developed by economists and economic geographers. The authors, however, claim that all aspects of agricultural geography are reviewed, including environmental problems. No doubt the precise balance between the physical, human and economic components will vary, but in this publication the economic side receives most of the emphasis, which creates something of an imbalance. At times one needed reassurance that the title of the book was indeed "Agricultural Geography" and not "Agricultural Economics". Chapter 1, "Nature of Agricultural Geography", which devotes six pages to "Economic Concepts and Principles" against approximately two pages given to "Physical and Biological Environment" and "Cultural Constraints" respectively, illustrates the point well.

Generally, the subject matter is developed logically and well throughout the book. Chapter 8, "Government Policy", provides an admirably concise exposition outlining the influence of government activity on the spatial organization of agriculture. Modern sociological approaches together with the effect of individual psychology are introduced when discussing decision making. Agricultural activity and physical environment, including land classification, receive scant attention.

A useful introduction into the problems of data collection, classification and the relevant analytical techniques is attempted in Chapter 9. This rightly points out that many approaches result in only crude general comparisons.

The book is written in a pleasing, easy style and is attractively produced with clearly presented diagrams and tables. *Agricultural Geography* is a stimulating book recommended to all interested readers and libraries.

B. WILKINSON

Amines and Membranes

Biogenic Amines and Physiological Membranes in Drug Therapy. By John H. Biel and Leo G. Abood. Part A: Pp. ix+1-160. \$12.50. Part B: Pp. vii+161-525. \$25.50. (Marcel Dekker: New York, November 1971.)

BOOKS consisting of a collection of review articles written by various authors often serve a useful purpose in integrating existing facts and new ideas into an exciting whole. Some are more successful than others, however; unfortunately,

these two volumes do not fall into the successful category. The title and the preface suggest a comprehensive coverage of effects of amines and therapeutic drugs on membranes; in fact the first book consists of four reviews on membrane structure and ATPase and on ionic movements in natural and artificial membranes, with very little correlation with either amines or therapeutic drugs, while the second has six review articles on biogenic amines, their uptake, synthesis, degradation and oxidation, with passing reference to therapy and virtually no connexion with physiological membrane properties. This lack of entity may explain why a 500-page text has been split into two volumes; the potential market for each separately may be greater than that of the whole.

It is not easy to see what this potential market would be. The blurb claims it is aimed at the research scientist, physiologist or pharmacologist, the medical student or the physician in clinical pharmacology. Individual reviews could be of great use to a variety of people, but severe warnings regarding copyright preclude the use of separate articles for teaching purposes or research. The cost would deter many, particularly students, from buying the books either singly or as a pair and only a few researchers would find more than three or four of the ten reviews of vital interest to their work. Also the appearance of the publication is not attractive; the pseudo-typescript format and the excessive number of typographical or spelling mistakes would suggest a hurried, unedited production, but the paucity of references after 1969 in the majority of reviews, with the exception of the two by Creveling, Daly and colleagues, would suggest that most of them were written at least some 18 months before publication. The multitude of errors is annoying rather than confusing, however.

The individual review articles are, however, of an adequate standard. The preface expresses the hope that the book will raise as many questions as it will answer; this is noticeable in some papers by the lack of any attempt to provide definite conclusions. In volume A on membranes, the article which most appealed to me, probably because it was deliberately limited in scope but therefore concentrated on one aspect, was by Bean, Shepherd and Eichner on the production and properties of excitable artificial membranes. This highlighted some of the properties of membranes and made more relevant other more wide-reaching papers, such as the article by Abood and Matsubara on membrane structure and Tasaki's ideas on the role of cations and proposed transitions between stable membrane states in the squid axon. Volume

B on amines includes updated and expanded revisions of earlier reviews by Hornykiewicz on dopamine, Iversen on biogenic amine uptake, Kopin on false transmitters and also a more limited review of histamine by Schayer. But the most interesting to me was the article by Jerina, Daly and Witkop on the molecular rearrangements occurring during enzymic oxidations, the so-called NIH shift. It is difficult to see the relevance of this paper to the rest of the book, but it assembles together and summarizes a considerable body of work around a definite topic and then suggests some ways in which further research is necessary, thus achieving concisely the objects of this type of review.

In summary, these volumes contain much worthwhile accumulated information in a series of review articles of variable quality, but as a book it lacks the cohesion and correlation necessary to make it a useful whole. It seems a pity that an excellent concept was not carried out more successfully; perhaps this is because the range of topics or the scope of the articles was too limited and production was started before sufficient conclusive evidence was available.

R. LAVERTY

Low Temperatures

Cryogenic Fundamentals. Edited by G. G. Jaselden. Pp. xiii+757. (Academic: New York and London, October 1971.) £11.50; \$33.

IN covering the fundamental aspects of the technology of cryogenics in a unified manner over the whole temperature spectrum below about 150 K, *Cryogenic Fundamentals* lives up to its name as a comprehensive textbook of over 700 pages. It is aimed to meet the requirements of engineers involved with equipment design and operation, and of research workers in cryogenic laboratories.

Looking back over the 1960s we have witnessed a constantly changing pattern in cryogenics and a widening range of commercially viable applications. The decade has included the development of high field superconductivity from scratch; of large hydrogen-fuelled rockets like the Saturn V vehicles and the completion of the first Apollo lunar landing missions; of marine transportation of liquid natural gas on a large scale; of cryo-surgery and cryo-preservation techniques in medicine; of the $^3\text{He}/^4\text{He}$ dilution refrigerator for continuous refrigeration below 1 K down to 0.01 K or lower; and so on. The 1970s

will no doubt see a continuation of this expansion.

The editor has therefore wisely chosen to cover areas of fundamental value and interest to both engineers and scientists. A number of acknowledged experts from Britain and the USA have contributed to the textbook with comprehensive chapters, each averaging some fifty pages. Throughout, the editor has maintained a sound balance between theory and practice, while strongly resisting lengthy excursions into the history of the subject. The latter has been taken care of by means of extensive lists of references.

In the first half of the book, refrigeration, heat transfer and insulation, fluid dynamics, and materials of construction are dealt with in considerable detail and length. More particular aspects are then covered including adsorption, expanders and pumps, superconductivity and instrumentation, together with an important chapter on safety. The final chapter is a convenient source of cryogenic data and includes a valuable introduction to the use of equations of state, like the Benedict-Webb-Rubin equation, for generating thermodynamic properties of cryogenic fluids in computer-aided calculations.

In conclusion, *Cryogenic Fundamentals* is a standard textbook for the technology of cryogenics in the 1970s. Its realistic approach is summed up by the following extract from the introduction, viz.: "A low temperature environment for any industrial process should no longer be regarded as a last resort but as a serious contender worthy of detailed economic analysis and forecasting". R. G. SCURLOCK

Unimolecular Theory

Unimolecular Reactions. By P. J. Robinson and K. A. Holbrook. Pp. xvii+371. (Wiley: London, 1972.)

THERE can be few topics in physical chemistry with quite the same fascination and complexity as the unimolecular reaction, and few that have been less adequately served in monographs. The result is such a build-up of uncollated and uncriticized experimental material that even the most venturesome author can hardly be expected to put matters right in a single volume.

This work, as is made plain everywhere except in its title, pretends to be neither a comprehensive treatise on unimolecular reactions nor an elementary introduction to the subject; rather it is a painstaking and, for the most part, attractively presented study of one

very particular aspect of the problem, the so-called Rice-Ramsperger-Kassel-Marcus theory and its experimental support. It begins with a readable survey of the early history of the field, gives a modicum of introduction to vibrational analysis and, by way of Slater's now somewhat unfashionable harmonic theory, arrives quickly at the main subject-matter, the RRKM treatment itself. This is presented in a lavishness of detail which those who have struggled with the early key papers will certainly appreciate; nevertheless, twenty years after the main developments and more than forty since its beginnings, a more critical and contemporary account of the subject would have been welcome. As it is, the predominant impression is of exegesis rather than evaluation, and this is to be regretted.

In fact, to anyone who has been captivated by the theoretical subtleties of the problem or who requires more than a bibliographic guide to the mounting volume of experimental material, the whole tenor of the book is likely to seem disappointing, not to say exasperating. Although at least half its content is theoretical in character, the mathematical presentation is throughout hesitant and has the deferential tone of the writer treading close to his own depth. There are long sections where the reader is led by the hand through energy-level summations, convolutions and the construction of rate expressions, even down to the fine structure of computer programs, but in more than one place such methods are demonstrated in great detail only to be shown a little later to be obsolete. Unfortunately the effect of this is more ritualistic than informative and seems nowhere to point, even tentatively, towards directions in which the subject might be advancing. This is perhaps the book's most serious shortcoming.

Even accepting the curiously narrow terms of reference which the authors have set themselves, there are serious lacunae and misdirections of emphasis. Slater's work is discussed only in barest outline and the important shift of interest towards his "new approach" in the early sixties is passed over with hardly a mention. Although the experimental data collected are extremely voluminous, there is virtually no photochemistry, nothing on shock-tube studies or molecular beams, nothing on tritium recoil reactions or those from excited states and—most surprisingly in view of the impressive success of the RRKM theory in predicting mass-spectral cracking patterns—nothing on charged particle systems. Little, in short, with the possible exception of a chapter on chemical activation, that strays far from the rather dogged tradition of thermal pyrolysis in a static system. Finally,

although the coverage of this kind of work is as compendious as any in print, it must be pointed out that a considerable proportion of the thermal reactions cited—Diehl's Alder rearrangements, decompositions of ethers, amines and the like—would scarcely pass the stringent tests for homogeneous unimolecularity on which the authors rightly insist earlier in the book.

It may perhaps be argued that a topic as difficult as chemical reaction theory is best approached through a concentrated treatment of one particular facet, especially if this is felt to be historically decisive. But a truncation of the problem in the interests of space alone with little attempt to condense or co-ordinate with the growing points of the subject seems to be of very limited value. The last ten years have seen marvellous innovations in chemical kinetic theory and experimentation alike. Barriers to communication with the field of physical scattering processes have been overcome; molecular and ion-beam experiments and trajectory studies of collision complexes have removed some of the distinction between uni- and bimolecular processes; laser photochemistry promises a completely new frontier. The detached reader, finding no inkling of all this in the present volume, might be excused taking a somewhat dim view of the present state of the subject. He would be advised to persevere.

M. R. HOARE

Human Evolution

Journal of Human Evolution. Edited by A. B. Chiarelli. Vol. 1. No. 1. Pp. 135. (Academic: New York and London, January 1972.)

WHILE a welcome should be extended to any new arrival on the scene of scientific journalism, the justification for the continued existence of a publication and for its status within this particularly narrow field must remain in abeyance. Two questions should be asked about any new scientific journal: is it needed and, if so, does it fulfil this need? Although a journal devoted to the study of human evolution would be a distinct and important addition to the periodical scientific literature, the new *Journal of Human Evolution* does not seem to fill this niche. The editor, A. B. Chiarelli, states that the journal will publish papers "concerning the physical and sociological evolution of non-human primates and the origin and evolution of man". Although the stated scope is a bit broader than the title would indicate, this editorial policy is borne out

by the content of the first issue. Of fourteen articles, seven are concerned either wholly or partly with man but only four of these deal with problems relating to human evolution. Of the remaining seven, five papers deal with problems in primate genetics and taxonomy; one further paper discusses archaeological dating techniques and the last is a study of drift in primate groups which "might provide us with a tool" in the analysis of certain hominid groups.

The book review section certainly is not *au courant*. One wonders what the point can be of reviewing a book published six years ago (*The Foundations of Genetics*, by F. A. E. Crew, 1966). The remainder, published in 1970, are merely 1.5 to 2 years old. The somewhat innovative "Book News" section, however, is useful. While none of the included books is particularly new, the device of presenting the table of contents is highly informative and lends objectivity to the inevitably subjective business of book reviews.

While a number of other regularly available periodicals publish this same range of material (and more recent book reviews) it must be pointed out that the quality of the articles in this first issue is high and they seem to be well researched and carefully presented. In addition, Chiarelli has assembled an editorial board which is impressive both in size and reputation.

GAIL KENNEDY

Polymer Handbook

Fibres, Films, Plastics and Rubbers: a Handbook of Common Polymers. Compiled by W. J. Roff and J. R. Scott. Pp. 688. (Butterworth: London, January 1972.) £15.

THE original version of the volume under review, entitled *Fibres, Plastics and Rubbers*, was compiled by W. J. Roff and published in 1956. It received wide and well-merited acclaim as a unique work of reference on the properties of polymers then in common use. Since that time, published information on polymers has proliferated, many new types have become commercially available, and polymers in multifarious forms have penetrated almost every area of scientific and industrial activity. Consequently the need became increasingly apparent for a detailed revision and expansion of the earlier work, a rather daunting task which was undertaken by the two senior compilers with the assistance of staff of the Shirley Institute and of the Rubber and Plastics Research Association. The completed

result of their collaboration has now appeared and in every way it can be regarded as a worthy successor to the original book.

All major classes of industrially useful synthetic and natural polymers are treated in the first forty-two sections, each dealing with a generic type and containing, in systematic order, detailed information on structure, chemical, general physical, thermal, mechanical and electrical properties; also given are brief notes on fabrication, serviceability, utilization and history. Separate discussion of individual properties and related topics occupies a further thirty-four sections, in most of which are given comparison tables listing polymers in order of magnitude of the respective property. The user can therefore rapidly scan the polymer-property matrix both horizontally and vertically. Sufficiently comprehensive lists of references are appended to each section, and indexes by author, trade-name and subject are provided.

The compilers have achieved a remarkable feat of compression, with a degree of attention to detail and manner of presentation that is nowadays rare. Accordingly, only relatively minor criticisms can be made. Perhaps the most serious of these is that the non-specialist reader is given insufficient warning of possibly large variations in polymer properties with temperature and time-scale of measurement. Although such effects are discussed in general terms, almost all the numerical data are single-point values; the inclusion of some figures illustrating time- and temperature-dependence (for example, creep) for some of the more common plastics would have served to emphasize this characteristic feature of polymer behaviour. Neither melt flow properties nor crystalline morphology are discussed. Metric units are used throughout, but appropriate conversion factors are supplied together with a short note on SI units.

P. F. ONYON

Living Mechanisms

Biomechanics: its Foundations and Objectives. Edited by Y. C. Fung, N. Perrone and M. Anliker. Pp. xiii+641. (Prentice-Hall: Englewood Cliffs, New Jersey, 1972.)

THIS book is the edited proceedings of a symposium of the same title held at the University of California in 1970. Biomechanics is presented as a subject of the moment, giving fresh impetus to biology and medicine as the old descriptive and phenomenological approach

becomes exhausted. Two-thirds of the contributors are engineers from departments of engineering or bioengineering and only one is from an institute that incorporates biomechanics in its title.

The main theme of the contributions is the mechanical aspects of cardiovascular and pulmonary structures, although skin, tendon, bone and the mechanics of injury also receive considerable attention. The contents of Parts 2 to 3 are very welcome both for their comprehensive recent bibliography and a presentation of how engineers approach the properties of biological structures and materials. One might have wished for a stronger dialogue between the biologist and the engineer. The methods of approach, both experimental and mathematical must surely yield practical returns, although more authors seem to be treading a path from organism to model at present than vice versa. Few review texts exist in these selected fields and the work deserves a place as a sourcebook in every department that is concerned with relationships between structure and mechanical function of biological material. Those in surgery and orthopaedics who are contemplating collaboration with bioengineers will also be repaid by study of the book because it will help them to ask the right sort of question.

Biomechanics, a term which would have implied or at least embraced studies of the intact body in the nineteenth century, is used here to describe a mixture of bioengineering and what was termed biophysics fifteen years ago. Students of physical education and kinesiology who turn to this book hoping to find the long-awaited and advanced treatment of what they call biomechanics will be disappointed. Muscle (apart from myocardium) rates only six pages, without a bibliography, and the mechanical analysis of body movement likewise receives scant attention, apart from an excellent chapter on biomechanics and the amputee.

The foundations of the subject are not discussed and the contribution to Part 1, which deals very readably with the objectives of the subject in physiology and surgery and rather obscurely with those in patient care and rehabilitation, surprisingly quotes only four publications before 1960. Physical scientists have contributed to physiology for a long time. It is more the appearance of the engineers as a professional group on the biological scene that is novel, rather than the approach. If muscle, for example, had received the review it deserves under the title of this book, it would have been more apparent that biomechanics developed in a century, not a decade.

D. W. GRIEVE

CORRESPONDENCE

Terrorism

SIR,—“The death of Dr Aharon Katchalsky in the wild shooting at Tel Aviv airport” provided *Nature* (237, 302; 1972) with a springboard from which to inveigh against what appears to be world-wide terrorism. Thus, the bombings by the Irish Republican Army, the killings in central Africa, the perennial threat of indiscriminate slaughter in the Middle East, and the hijacking of an airplane in an attempt to secure Angela Davis's release from prison are cited as particularly heinous examples of terrorism that usually are counterproductive to the cause for which the acts were performed.

Remarkably, no mention is made of the far greater terrorism that has been practised by the United States in Vietnam for almost a decade. It is difficult to imagine terrorism that is more senseless and counterproductive than that which the US has inflicted on the Vietnamese people, yet *Nature* does not see fit to include that activity in its extended list. Does not *Nature* consider massive indiscriminate bombing and shelling, widespread defoliation, and untold numbers of massacres of the kind that took place at My Lai as wanton terrorism? Or is it that when millions (rather than dozens or hundreds) of people are killed, maimed or made homeless by these acts that it is no longer terrorism but something more acceptable? Or can it be that when these kinds of acts are perpetrated in the name of such a “civilized” and distinguished nation as the United States by definition they no longer can be considered acts of terrorism?

I write to raise this issue because I fear that not to cite the grossest form of terrorism now being exercised is to imply that *Nature* approves of US actions in Vietnam and such approval can only help to extend the devastation in that long suffering land. The last thing needed is more approval of US activities in Indochina.

Yours faithfully,

LESTER GOLDSTEIN

College of Arts and Sciences,
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Boulder, Colorado 80302

The omission of Vietnam from the list of examples was deliberate but not intended as an implicit comment on United States activities in Vietnam (which are, in any case, not part of *Nature's* parish). But what has happened and is happening in Vietnam

differs from acts of terrorism such as that in which Aharon Katchalsky was shot in that it is a consequence of a declared military policy of a government.—Editor, *Nature*.

Science and Ethics

SIR,—All scientists who are concerned with the ethical aspect of their profession will be interested in the problem raised by the Brain Research Group of the Open University (*Nature*, 237, 469; 1972) about the NATO sponsorship of a summer school on brain proteins due next September. Since science has ceased to be a gentleman's hobby it has lost most of its purity and independence. We are facing a number of promises and threats brought about by the social involvement of science, grown too fast to allow the development of new social ethics. The “bad” exploitation of scientific achievements is possible under more or less disguised forms and the warning of Rose's group seems justified.

My participation as a student at two previous NATO summer schools on brain research gives me the opportunity to contribute some facts to this problem.

The meeting in Milan in 1970 (Chemistry of Brain Development) was attended by thirty-five speakers (twenty-five from western Europe and ten from the USA) and by fifty participants (western Europe twenty-eight, USA fourteen, India three, eastern Europe two, Japan two, Greece two, Egypt one, South Africa one, Argentina one, Cyprus one, Australia one, Mexico one, Pakistan one, Venezuela one, Spain one). The meeting at St Vincent in 1971 (Structural, Functional and Chemical Characteristics of Developing Neurons and Neuron Sets) was attended by eleven members of faculty (five from USA, five from western Europe, one from eastern Europe) and by sixty-one participants (western Europe thirty-six, USA sixteen, eastern Europe seven, Australia two, Japan two, Republic of China two, Canada two, Israel one). Both schools were also sponsored by scientific societies and a private firm. Free scientific communication was allowed as in any other international meeting (more, in fact, because of the great deal of faculty-student interaction). Scientific information and ideas exchanged during the meetings will of course be available for military purposes (like all data published in bio-

logical journals) until we decide to stop making war. The motivations and the purposes of the sponsors have not been declared at the meetings, NATO being hardly mentioned in the opening speech. The purposes of NATO's Scientific Affairs Division must have been defined at the moment of its establishment, and probably they are (on paper) very similar to those of other international organizations like EMBO and ECBO.

It is my opinion that the participants of these meetings have not in any way facilitated the military application of neurobiology. The presence among faculty and students of people coming from, and working in, Communist countries (as in any other scientific meeting) is proof of no direct or indirect military purpose; it also shows that scientists are not affected by the international neurosis of the blocs, and get together to solve their puzzles (even at the expense of a mission-orientated organization). I support the suggestion of Rose's group that NATO's scientific sponsorship is a tip to obtain a kind of social respectability (not legitimation, which is provided by human aggressiveness). Therefore a question of principle would justify deserting these schools to show political disagreement with their financing organization. On the other hand, both NATO and our laboratories share the same financial backing and political will (British taxpayers and electorate). So we should all pack up and go on strike until our governments have stopped all kinds of military activity. I would rather join an international movement of scientists aiming at this than try to tell military biology from peaceful biology, or devise tricks to stop generals from learning anything about the metabolism of myelin proteins.

Yours faithfully,

PIERO P. GIORGI

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Publication Delay

SIR,—Permit me to take issue with your correspondents Gassman, Penney and Khan (*Nature*, 237, 58; 1972) on the matter of publication periods in specialized scientific journals. My company publishes some 130 journals serving many special fields of science. Of these, over half appear exclusively or mainly in English, while the remainder are always pleased to have articles in

English. It has been our constant endeavour to trim publication periods, and manuscripts average 4-5 months from acceptance to publication. We are currently streamlining our production procedures in a way which will cut this period to 2-3 months.

There remains the time required to review and, if necessary, revise manuscripts. Although we impress on editors and referees the need for speed, this period is to some extent beyond our control; however, it should be clear that

without it we could not maintain standards at the highest level.

We already implement the suggested solutions to the maximum practicable extent: (i) large page size, frequent issues; (ii) review by local editors; (iii) air mailing; (iv) lists of forthcoming articles and, in some cases, prepublication abstracts in leading abstracts journals. The writers will surely appreciate that, if we were to make manuscripts freely available prior to publication, we should be in danger of

sawing off the branch we (as producers) and they (as users) are sitting on.

Finally, we, too, are aware that all these measures entail extra costs, and frequently must result in higher subscription rates. Perhaps the proponents of rapid publication would make it their special task to explain this to librarians.

Yours faithfully,

H. MAYER-KAUPP

*Springer-Verlag,
69 Heidelberg 1*

Obituary

Dr J. R. Rossiter

DR JOHN REGINALD ROSSITER, Director of the Institute of Coastal Oceanography and Tides, died on May 16, 1972, at the age of 52.

The name of Jack Rossiter will always be associated with the Liverpool Observatory and Tidal Institute on Bidston Hill, which he joined as a junior assistant in 1937. Apart from a period on active service with the Royal Artillery during the Second World War, he spent his entire working life at Bidston Observatory in the course of which he earned an international reputation for his researches in the field of tides and storm surges. His career was bound closely to the fortunes of the institute to which he belonged. He received no formal university training but by part-time study obtained a London (external) BSc degree, with first-class honours, in 1947. The same University awarded him an MSc degree in 1956 for a thesis entitled "The Meteorological Disturbances of Water Level." His DSc degree followed in 1961.

In his early years at Bidston, Rossiter was intimately involved in the processes of tidal prediction and analysis for which the Institute had become renowned under its director, Dr A. T. Doodson. This brought him into direct contact with observational data and its treatment employing numerical methods of skilful and ingenious design, in which elaborate and advanced computations were carried out with no more than desk calculators. He often recounted with pride the high standards of accuracy and efficiency to which he and the other assistants were committed in those days, and in directing the preparation of tidal predictions in later years insisted on the maintenance of such standards.

By 1952, the work of Professor J. Proudman in Liverpool, and that of Dr Doodson and Dr R. H. Corkan at Bidston, had made the institute a world

centre for research in tidal dynamics. The sudden death of Corkan at this juncture, and the illness of Doodson which was to follow, gave Rossiter responsibilities and opportunities to continue work in the rich tradition which had been built up. He became interested in the determination of tidal charts by solving finite-difference forms of the hydrodynamical equations using the relaxation method, again involving hand calculations. In collaboration with Doodson, charts of the M_2 tide were derived for the Irish Sea on the basis of coastal observations of tidal elevation only. Later he applied the relaxation method to tides in an idealized ocean bounded by meridians and parallels.

Following the disastrous North Sea storm surge on the night of January 31, 1953, which caused widespread coastal flooding in south-east England and Holland, it fell to him to investigate the surge observationally and thereby assess its origins and development. This he did in an outstandingly comprehensive paper to the Royal Society. In 1954 the Advisory Committee for Oceanographical and Meteorological Research, under the Ministry of Agriculture and Fisheries, was set up and under its jurisdiction the institute was authorized to undertake a programme of research work on the subject of storm surges. Rossiter became a prominent member of the committee and contributed much to its deliberations and decisions, taking a special interest in the North Sea surge problem. He developed methods of forecasting storm surges on the east and south coasts of Great Britain employing statistical formulae. Subsequently he turned his attentions to the propagation of tide and surge in the Thames estuary, and in numerical solutions of the hydrodynamical equations was able to demonstrate and explain the interaction between these waves. Following this investigation he worked with G. W. Lennon in constructing a numerical

tidal model of the Thames employing the initial-value method. The treatment given was so thoroughly professional as to make it a standard reference for subsequent work of its type.

An assessment of his scientific contribution would not be complete without special reference to his studies of mean sea level and its variations. For many years he was secretary of the Committee on Mean Sea Level of the International Association of Physical Oceanography, and from 1960 was Honorary Director of a permanent service for the analysis of sea level data, centred at Bidston. In these capacities he was responsible for the compilation and publication of monthly and annual values of mean water level, and also produced a bibliography on the subject. Taking data for European waters he resolved annual values into a secular variation, wind and air pressure contributions, and the nodal tide. This made possible the definition of a mean sea level surface of reference for the region. A result he found which deserves mention for its practical importance in the design of sea defences against coastal flooding is the tendency in the Thames estuary over the last 50 years for the height of mean high water to increase at approximately twice the rate at Tower Pier (over 2 ft/century) than at Southend, this being chiefly due to the relative rate of increase in tidal amplitude at the two places.

After the retirement of Doodson in 1960, the Tidal Institute became a department of the University of Liverpool with Rossiter as its director. Once in a position of authority, he started to give the institute a new and more expansive outlook. Research and scholarship remained vitally important ingredients, but, in addition, a go-ahead thrustful approach was introduced which sought to give the institute a more essential and participating role in marine science.

One of his first objectives was to use the digital computer for tidal analysis

and predictions. By that time these machines had developed sufficiently to be able to compete in efficiency with the analogue tide predictors. The conversion from the traditional routines was achieved and new fields of investigation were opened up in the application of the computer to the analysis of tidal data. He carried out, with Lennon, an intensive analysis of shallow-water tides which led to a method of harmonic tidal prediction using an extended number of constituents. He was quick to recognize the potential value of numerical modelling techniques in oceanography and encouraged the development of a substantial effort in this sphere for which the institute has become well known. Part of the research programme he directed was concerned with the design and operation of tide gauges, and in this he always regarded the project for an off-shore gauge to be one of very special importance.

In April 1969 the Tidal Institute was transferred to the Natural Environment Research Council and was renamed the Institute of Coastal Oceanography and Tides. Under Rossiter's guidance a rapid expansion of staff numbers and facilities followed. As a consequence, much of his time had to be spent in establishing and administering the emerging institute—charged to cover a wider area of study in coastal oceanography than before. Those who worked closely with him realized that he had taken on a strenuous and exacting task, but scarcely appreciated then the toll it must have been taking of his strength. However, he had hopes and dreams for the future of his group, and it is sad that he did not enjoy the normal life span to see the fruits of his labours.

He was an international scientist, particularly exerting an influence on the affairs of the International Union of Geodesy and Geophysics. At home he gave service on numerous committees dealing with the progress of oceanography and hydraulics. His dynamic friendly presence will be missed by many.

Announcements

University News

Dr Isaac Goodman, of ICI, has been appointed professor of polymeric materials at the **University of Bradford**.

Professor A. R. Rogers, Department of Pharmacy, has been appointed dean of the Faculty of Science in **Heriot-Watt University** and **Professor T. D. Patten**, Department of Mechanical Engineering, has been appointed dean of the Faculty of Engineering.

Dr P. Grootenhuys has been appointed to the chair of mechanical engineering and **Professor N. H. March** to the chair of theoretical solid state physics, both tenable at Imperial College, **University of London**; **Dr P. A. Jewell** has been appointed to the chair of zoology tenable at Royal Holloway College and **Mr K. E. F. Hobbs** has been appointed to the chair of surgery tenable at the Royal Free Hospital School of Medicine.

Mr Henry T. Howat has been appointed to a personal chair of gastroenterology in the **University of Manchester**.

Dr K. G. Webb has been appointed to the James Spence chair of child health in the **University of Newcastle upon Tyne**.

Dr David Hull has been appointed to the Foundation chair of child health at the **University of Nottingham**.

Dr K. J. Barnes has been appointed to the chair of theoretical physics and **Dr R. Postlethwaite** to the chair of microbiology in the **University of Southampton**.

Miscellaneous

The **International Meteorological Organization** prize has been awarded to **Academician Viktor Antonovich Bugaev**, director of the Central Institute of Prognoses, Moscow.

The **Akzo award** for scientific research, made annually by the Dutch Society of Sciences, has been won this year by **Dr H. C. Volger** of the Royal Dutch/Shell Laboratories, Amsterdam, for his work on catalysis and organic coordination chemistry.

International Meetings

July 10–12, **DNA Synthesis *in vitro***, Madison (Mrs Mary Parker, Steenbock Symposium, Department of Biochemistry, University of Wisconsin, 420 Henry Mall, Madison, Wisconsin 53706, USA).

July 10–12, **Vacuum Microbalance Techniques**, Uxbridge (VTM Administration Office, c/o The Scientific Instrument Centre, 1 Leeke Street, London WC1).

July 10–13, **Vacuum Instruments and Methods in Surface Studies**, Guildford (The Secretary, British Vacuum Council, 47 Belgrave Square, London SW1X 8QX).

July 10–14, **Nitrogen Metabolism in Plants**, Leeds (Mr J. T. Gleave, Department of Adult Education and Extramural Studies, University of Leeds, Leeds LS2 9JT).

July 10–14, **Enzymes and their Use in Analysis and Clinical Diagnosis**, Cambridge, Massachusetts (Office of the Summer Session, Room E19–356, Massachusetts Institute of Technology, Cambridge, Massachusetts 02139, USA).

July 10–14, **Interaction of Radioactive Contaminants with the Constituents of the Marine Environment**, Seattle (Miss C. H.

de Mol van Otterloo, Division of External Relations, International Atomic Energy Agency, Karntner Ring 11, PO Box 590, A-1011 Vienna, Austria).

July 11, **Fermentation Pilot-Plants**, Leeds (Mr R. Elsworth, New Brunswick Scientific Co. Inc., Salisbury, Wiltshire).

July 11–13, **Society for Experimental Biology Meeting**, Dundee (Dr A. P. M. Lockwood, Department of Oceanography, University of Southampton, Southampton SO9 5NH).

July 12–14, **Visual Performance or Preference**, Cardiff (IES, York House, 199 Westminster Bridge Road, London SE1 7UN).

July 13–15, **Cytogenetics in Relation to Plant Breeding**, Aberystwyth (Professor D. A. Hopwood, John Innes Institute, Colney Lane, Norwich NOR 7OF).

July 14, **Bone and Tooth Society Meeting**, Bristol (Dr M. V. Stack, MRC Dental Unit, Dental School, Lower Maudlin Street, Bristol BS1 2LY).

July 16–22, **Public Health Engineering in Developing Countries**, Loughborough (John Pickford, Department of Civil Engineering, University of Technology, Loughborough, Leicestershire).

July 17–18, **Failure of Fibre Reinforced Composite Materials**, Guildford (Meetings Officer, Institute of Physics, 47 Belgrave Square, London SW1X 8QX).

July 18–21, **Hormones, the Brain and Behaviour**, London (Dr R. P. Michael, Institute of Psychiatry, De Crespigny Park, Denmark Hill, London SE5 8AF).

July 19–21, **Strength and Microstructure of Non-Metallic Materials**, Cranfield (Meetings Officer, Institute of Physics, 47 Belgrave Square, London SW1X 8QX).

July 19–21, **Defects in Semiconductors**, Reading (Meetings Officer, Institute of Physics, 47 Belgrave Square, London SW1X 8QX).

July 19–22, **Calorimetry**, Utah (J. M. Sturtevant, Department of Chemistry, Yale University, New Haven, Connecticut 06520, USA).

July 23–28, **Pharmacology**, San Francisco (Secretariat, Fifth International Congress on Pharmacology, 9650 Rockville Pike, Bethesda, Maryland 20014, USA).

July 24–27, **Organic Chemistry of the Excited State**, Reading (Dr J. F. Gibson, The Chemical Society, Burlington House, London W1V 0BN).

July 25–29, **Production and Utilization of Sunflowers**, Clermont-Ferrand (Congrès-Services, 1 Rue Jules-Lefebvre, Paris 9, France).

July 31–August 5, **International Union of Amateur Astronomers General Assembly**, Malmö (Malmö Astronomi- and RymdfartsSällskap (MARS), Box 250 60, S-200 47 Malmö 25, Sweden).

Reports and Publications

not included in the *Monthly Books Supplement*
Great Britain and Ireland

Maya Hieroglyphs Without Tears. By J. Eric S. Thompson. Pp. 84. (London: British Museum, 1972.) 80p. [174]
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Report of an Enquiry Into the Use of Academic Staff Time. Pp. 36. (London: The Committee of Vice-Chancellors and Principals of the Universities of the United Kingdom, 1972.) 30p. [184]
What Future for the Peak National Park? Pp. 39. (Bakewell: Peak Park Planning Board, 1972.) [194]
Technology and Man. By Sir Brian Flowers, FRSE. (First Leverhulme Memorial Lecture.) Pp. 29. (Liverpool: Liverpool University Press, 1972.) [194]
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Department of Education and Science. Statistics of Education 1970. Vol. 4: Teachers. Pp. xx+68. (London: HMSO, 1972.) £1.70 net. [204]
The Craig Affair: The Background to the Case of Dr. David Craig and others, University of Lancaster. Pp. 28. (London: Council for Academic Freedom and Democracy, 1972.) 20p. [204]
Furniture Industry Research Association. Eleventh Annual Report, 1971. Pp. 24. (Stevenage, Herts: Furniture Industry Research Association, 1972.) [204]
Civic Trust Awards 1971—County Council Areas. Pp. 122. (London: Civic Trust, 1972.) 75p. [214]
Shoe and Allied Trades Research Association. Annual Report 1971. Pp. 129. (Kettering, Northants: Satra, 1972.) [214]
The International Journal of Nautical Archaeology and Underwater Exploration. Vol. 1, March 1972. Edited by Joan du Plat Taylor. Published annually. Pp. 247. (London and New York: Seminar Press, 1972.) £2.50 UK; £2.90 overseas. [244]
The Annual Report of the Committee of Visitors of The Royal Institution of Great Britain, the Statement of Accounts and Other Reports for the year ending 31st December 1971. Pp. 64. (London: The Royal Institution of Great Britain, 1972.) [244]
Garlic as an Insecticide. By David L. Greenstock and Querubin Larrea. (Report No. 2.) Pp. 12. (Baintree, Essex: Henry Doubleday Research Association, 20 Convent Lane, Bocking, 1972.) 12p. [244]
Resuscitation. Vol. 1, No. 1, March 1972. Pp. 1-84. Published quarterly. Annual subscription £8. USA and Canada \$27. Single issues £2.50, USA and Canada \$7.50. (London: The Middlesex Publishing Company, 1972.) [244]
Getting the Most Out of Food. (The Seventh in a Series of Studies on the Modern Approach to Feeding and Nutrition.) Pp. 95. (London: Van den Berghs and Jurgens, 1972.) [254]
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Agricultural Research Council. Annual Report, 1970/1971. Pp. vii+67. (London: HMSO, 1972.) 47p net. [284]

Other Countries

US Department of the Interior: Geological Survey. Bulletin 1318: Geochemistry and Geology of Deep Drill Holes at Iron Canyon, Lander County, Nevada. By Ted G. Theodore and Ralph J. Roberts. With a section on Geophysical Logs of Drill Hole DDH-2. By Charles J. Zablocki. Pp. iv+32. (Washington, DC: Government Printing Office, 1971.) \$1. [203]
US Department of the Interior: Geological Survey. Bulletin 1063-M: Geology of the Flint Hill Quadrangle, Fall River County, South Dakota. By Henry Bell and Edwin V. Post. Pp. v+505-586+plates 32 and 33. Bulletin 1344: Geochemical Distribution of Some Metals in the Moenkopi Formation and Related Strata, Colorado Plateau Region. By Robert A. Cadigan. Pp. iv+56. 35 cents. Water-Supply Paper 1937: Surface Water Supply of the United States 1960-65. Part 16: Hawaii and other Pacific Areas. Pp. ix+710+plate 1. Professional Paper 633: Geology of the Florence Area, Wisconsin and Michigan. By Carl E. Dutton. Pp. iv+54+plates 1-8. Professional Paper 640-D: Ostracoda of Late Eocene Age from Eua, Tonga. By Joseph E. Hazel and John C. Holden. Pp. vii+11+5 plates. 50 cents. Professional Paper 640-F: Eocene Brachiopods from Eua, Tonga. By G. Arthur Cooper. With a summary statement on Teleost Otoliths by John E. Fitch. Pp. v+9+1 plate. 45 cents. Professional Paper 649: Mafic-Ultramafic Igneous Rocks and Associated Carbonates of the Gem Park Complex, Custer and Fremont Counties, Colorado. By Raymond L. Parker and William N. Sharp. Pp. viii+24+2 plates. Professional Paper 658-E: Mesozoic Stratigraphy of the Patagonia Mountains and Adjoining Areas, Santa Cruz County, Arizona. By Frank S. Simons. Pp. iii+23. 40 cents. Professional Paper 711-A: New Late Mississippian Ostracode Genera and Species from Northern Alaska. By I. G. Sohn. Pp. iii+24+9 plates. 70 cents. Professional Paper 712-A: Hydrology of Project Gnome Site, Eddy County, New Mexico. By James B. Cooper and V. M. Glanzman. Pp. iii+24+1 plate. \$1. Professional Paper 715-A: Combined Ice and Water Balances of Gulkana and Wolverine Glaciers, Alaska, and South Cascade Glacier, Washington, 1965 and 1966 Hydrologic Years. By Mark F. Meier, Wendell V. Tangborn, Lawrence R. Mayo and Austin Post. Pp. iii+23+6 plates. Professional Paper 778: Embudo, New Mexico, Birthplace of Systematic Stream Gaging. By Arthur H. Frazier and Wilbur Heckler. Pp. iii+23. 45 cents. (Washington, DC: Government Printing Office, 1971.) [213]
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World Health Organization. Technical Report Series, No. 488: Evaluation of Food Additives—Fifteenth Report of the Joint FAO/WHO Expert Committee on Food Additives. Pp. 41. (Geneva: WHO, London: HMSO, 1972.) 4 Sw. francs; 40p; \$1. [273]
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